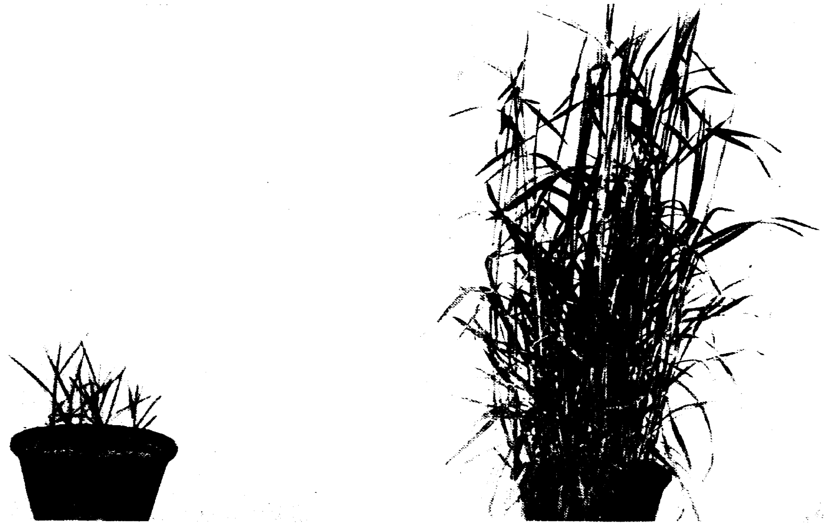

Barley growing in sand pot cultures. Conditions are equal in these two pots except that in the unhappy-looking one, no phosphatic plant food was added. (Courtesy of Long Ashton Research Station.)

CHEMICALS, HUMUS, AND THE SOIL

A Simple Presentation of Contemporary Knowledge and Opinions about Fertilizers, Manures, and Soil Fertility

by

DONALD P. HOPKINS

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PREFACE

THIS BOOK is not primarily a book for scientists or technicians of agriculture. It is written for the ordinary man who is interested one way or another in the care of the soil and the raising of crops, and it deals as non-technically as possible with chemical fertilizers and natural manures and their functions in crop production. There is a quotation from Hippocrates which expresses the purpose of this book: "If you miss being understood by laymen, you will miss reality." Whether the book achieves such a purpose only readers can judge.

It follows, therefore, that those who are scientists or technical workers in this field may find that quite a good deal of this book is not very new to them; and some of it more lengthily explained than they would wish. They must not feel aggrieved about that. For them there are already many excellent treatises and textbooks, while there are far too few *popular* books for people who are experts in quite other walks of life. Yet, after all, most of our soil is in the care of ordinary people.

It is not enough for the scientist to say that such-and-such a practice is necessary for this-or-that agricultural purpose. If the farmer or gardener is to carry out this practice effectively, he will do it all the more thoroughly if he knows *why;* and soil science is not so complex that it cannot be explained and discussed with a minimum of jargon. Much of the controversy about chemical fertilizers springs from the gap of understanding between scientist and layman. Those who have fiercely attacked fertilizers in recent years have taken their case to the layman, and it is not enough for orthodox scientists to retort that these matters have been settled for years. Perhaps it is true that they have indeed been settled for a long time, but the ordinary man

is not so sure because, unlike the scientists, he does not know which journals contain the authoritative accounts of research work, nor would he find these accounts simple reading if he tackled them. Responsible people tell him that fertilizers poison the soil, increase disease, and reduce nutritional values, and a case that he can understand within its own terms is set out. It is necessary for orthodox science to be similarly presented.

The first part of this book deals with the case for the use of fertilizers, and also with the general principles of soil fertility maintenance. The second part examines the case against the use of chemicals. To the limit of possibility, these matters are all presented in simple terms. Here and there details have been oversimplified, e.g., the account of the statistical method of assessing plot-test results. I hope no critic or opponent will seize upon these oversimplifications, for they are merely expedient short cuts which in no way damage the fundamental argument. Where I have ventured upon an opinion which is personal rather than general and orthodox, I have tried to underline it, not to draw attention to my own speculations, but to release anybody else from any implied responsibility.

The main theme of the book is to explain WHY rather than to say HOW. Some readers might be disappointed that there has been so little room left for practical details. To be frank, I consider that there have been almost too many books devoted to the rule-of-thumb business of how and how much, so many hundredweights * to the acre or ounces to the square yard. Empirical details are things to be looked up in more concise books than this. The principles that underlie all these details, these are matters that must be explained and discussed with the fullest freedom. However, in some effort to offset this lack of practical detail, a chapter about the commonly used fertilizers has been included in Part I, and an appendix on general fertilizer recommendations for various crops has been put at the end.

Finally, I must thank a large number of people who have knowingly or unknowingly helped me to write this book. The

* Hundredweight (cwt.) = 112 pounds.

Bristol Public Library for rendering invaluable assistance whenever I have asked, E. C. Large for permission to quote considerably from his book *Advance of the Fungi,* the executors of the late Professor Susan Stebbing for the use of quotations from *Thinking to Some Purpose,* Michael Graham for permission to quote from *Soil and Sense,* Lt.-Col. E. Parbury and Lt.-Col. G. Pollitt for permission to use letters previously published in a correspondence column debate, and many others whose words and wisdom I have used. Publishers connected with these borrowed words must equally be thanked. Where I have not been courteous enough to seek permission in advance, I hope there will be no sense of injury, for all quotations have been made for the purposes of research or criticism and with no intention to offend the laws of copyright.

Some of the chapters of this book appeared in more condensed forms in the pages of *The Fertilizer Journal* and *The Estate Magazine.* I have to acknowledge my indebtedness to the editors of these journals.

People who have bought this book to read something about the soil will be feeling defrauded if I continue this cavalcade of gratitude. It is inevitable that anyone who tries to explain some branch of science must rest with every word upon the shoulders of others. Nevertheless, despite this, it should not be supposed that anybody but the writer is responsible for the book. I say this not to claim any credit but to accept full responsibility in the event of discredit.

D.P.H.

PREFACE TO THE AMERICAN EDITION

THE ORIGINAL preface to the first English edition was, of course, written before either author or publisher had any certain idea about the book's reception. In that preface I disclaimed any intentions of aiming the book at agricultural scientists, and said with some emphasis that it was a book for the *layman,* for the practical man who handles the soil and raises crops. However, it was very soon evident from the response to the British publication that many scientists had ignored my attempt to *sign off*. What I had failed to realize at the time (1944/45) was that, owing no doubt to the enormous mass of research work and farming data about soil fertility, fertilizers, and manures, scientists as well as *laymen* would find some use and virtue in a book that tried to present a broad picture of current views in relatively simple terms.

Indeed, there was at first some indication that the book had missed its intended target and hit another. For some scientific writers, favorably reviewing it in technical journals, expressed doubts that those whom I called *laymen* would be able to cope with its matter. Their argument seemed to be that if they liked it, other non-scientific people probably would not, because its technical level must be too high-pitched. This verdict of the critical columns was depressing, but fortunately my disappointment did not last for long. It took farmers and growers rather more time to digest the book, and they were not so ready to express their reactions and opinions, but in due course, in a matter of a few months, their letters and their queries came along, and it was more than obvious that the book had not been too stiff or too academic for my *laymen*. Indeed, some of the subsequent correspondence I have had with progressive modern

farmers has been, I am sure, of more value to me as an agricultural scientist than it has been to them. More than ever I am convinced that the scientist must not inhibit himself with the false idea that this or that piece of technical information will be *above the heads* of the non-scientific, and instead he should hold on to the true idea that, if he expresses himself clearly and simply, there will be no gap and little time-lag between the research station and the farm, at any rate the progressive farm. And more than that, once the farmer really grasps, really understands, what the scientist is driving at, then he will often help by reporting practical evidence that is highly relevant.

I thought it worth while to report this post-publication experience not merely from the viewpoint of agricultural science but because it has wider implications. So-called *popular* science is of vital importance to the technical world of today. It is utterly wrong to think of *popular* science as something written down to some average man with the mentality of a child of about ten; or as something that must be written up with sensational headlines and frequent interjection marks to indicate miracles. It must be a serious branch of science, handled perhaps by a somewhat specialized type of scientist, a man who does not forget that the layman and the scientist have both been born with about the same quota of gray matter, and who remembers that technical jargon in nine cases out of ten is merely a shorthand device to save scientists' time and not a compulsory language. We must realize that today every intelligent *layman* is some sort of a specialist himself, whatever job he does, one or another branch of applied science inevitably impinges upon his work and his plans. Yet far too many scientists still feel that they lose dignity and caste if their pronouncements do not need a glossary, and when now and again a scientist (often a young one) ventures to write a useful and *popular* article, he earns the frowns of his older colleagues as well as a few dollars. For all that, there never was a time when it was more vital for science and scientific objectives to be understood, for what science can and can't do to be generally realized.

There are one or two other points I should add to this preface. The book inevitably has a British angle. I do not mean a nationalist angle, I mean that its background and a good deal of its documentary evidence has been that of our British agriculture. However, much of the confirmatory evidence has come from American research reports or from records from other parts of the world. It would have been quite wrong of me to attempt to *Americanize* the book, to replace British examples with American examples, and so on; for I should then have been discussing things I do not know instead of writing about matters with which I have reasonably direct contact. Also the book was written during the last 2 years of the recent war, and here and there will be found references that are no longer as topical as they then seemed. Apart from one or two paragraphs that were really badly *dated* by this tendency, I have, however, left the text unchanged. To some chapters I have added a few paragraphs, further evidence on certain points having come forward since the first publication in England.

In any case, this question of soil fertility and the functions of fertilizers and organic manures is much the same question in America as in Britain. The American problem differs only by being far more troublesome in certain areas, the soils having more extreme climates and past histories of more severe cropping. If a British book can help in any way toward clarifying controversy in America about the ways and means of maintaining soil fertility and health, it will be but a trifling return for the vast contributions of produce from American soil that came to Britain when she most needed it, indeed, not infrequently, the pages of the original manuscript of this book were written and typed with energy derived from American canned and lease-lent foods.

D.P.H.

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PART ONE

THE CASE FOR FERTILIZERS

CHAPTER I

APPROACH

"To think effectively involves knowledge of the topic, dispassionateness in weighing the evidence, ability to see clearly what follows from the premisses, readiness to reconsider the premisses if necessary, and, in short, courage to follow the argument *to the bitter end,* if the end be indeed bitter." The late PROFESSOR SUSAN STEBBING, *Thinking to Some Purpose.*

IN RECENT times we have become far too inclined to think lightly and casually of man's age-old and first-priority problem, the achievement and maintenance of soil fertility. We have thought ourselves and our times to be concerned with other problems, with industrialization, with distribution, with the economics of money, with one storm after another in our twentieth-century teacup. That we have all these other problems to face is undeniable—no one who has lived through both world wars and the turmoils of the interlude between could sanely seek to minimize them. However, caught in the muddled web of all these maladjustments, we have paid an all too scanty attention to the soil. In times of war one remembers that an army marches on its stomach and circumstances then force us to think about food and its production; in peacetime, in normal peacetime anyway, most of us are content to think of food as something that we can buy fairly cheaply in shop or restaurant.

Until 1939 Britain, a country that relied upon importing more than half of her foodstuffs, was staggeringly complacent about her soil. The repeal of the restrictive Corn Laws in 1846, ostensibly caused by the potato famines of Ireland but also influenced by the back-stage desire of manufacturers to introduce free trade, brought into British mentality the idea that plenty of food was

always grown abroad whatever happened to the crops grown at home. A nation hitherto both sea-faring and soil-tending shifted the balance toward the ships, with cargoes of out-going pottery and textiles and machines set against cargoes of in-coming food. World War I temporarily disturbed this paradise of simple commerce; and World War II has possibly shattered it. In the war periods imported food no longer depended upon the mere hiring of ships. Ships could be sunk and seamen drowned. We need not expand or emphasize this bitter point. It is both past and recent history etched in our hearts, and those who do not already realize it to the full never will. In both the wars British soil was suddenly called upon to tackle the job of feeding the whole population, a task which had not been its exclusive or near-exclusive honor since 1846, and the conditions were very different from those in 1846. Industrial expansion had brought prosperity, and, even though that prosperity had not been evenly shared, the population had vastly expanded. There were far more bellies to be filled than in the good old days when the corn harvest mattered more than a rise or fall in Consolidated Tobacco or Amalgamated Aspirin. In 1846 Britain measured about 19 millions; in 1914, 41 millions; and in 1939, 45 millions. Of course, it was impossible to provide enough food from British soil, but a remarkably high degree of food self-sufficiency was achieved. The sugar rations were wholly maintained by the beet crops, and potatoes, so often and unsuccessfully stood in line for in 1914–18, were, save for one brief period of scarcity in 1945 due to bad weather during harvesting, always liberally available. Indeed, between 1939 and 1945 enormous acreages of land long considered fit for nothing but weeds and rabbits were brought, admittedly by intense human effort, into a high state of fertility.

In America, too, an increasing consciousness of the needs of the soil has been forced into the mixed bag of burning issues that we call public opinion. There it was not so much the wartime demand for food that broke up complacency—the more gradual and perhaps more deadly scourge of soil erosion reminded men that, for all their factories and their cars and their

radio-sets, they still lived primarily upon their own soil and what it could produce. It is possibly true to say that the "Dust-Bowl" to America was a warning much the same as the U-boat to Britain. The outstanding work of the United States Soil Conservation Service and its Chief Officer, Hugh H. Bennett, has underlined the essential meaning of soil fertility not only in the United States but in every country in the world where erosion sweeps away, or threatens to sweep away, man's agricultural conquests.

In Australia, New Zealand, Canada, South America, China, India, South Africa, and in some parts of Europe, in all these food-producing areas of the world, what we call soil fertility has suddenly or gradually been lost by the process of erosion to a total sum of many millions of acres. When it is considered that not much more than a tenth of the world's land surface is blessed with a top-soil layer suited to agricultural use, and that the world's population is still expanding at an unprecedented rate (it has increased by about 500 millions since the beginning of this century), how can any thinking person dare to feel complacent about this business of soil fertility?

Most people will certainly have come across some highly controversial arguments about the *right* way to approach the problem of fertility. There have been innumerable articles in newspapers and journals, there have been critical books denouncing the current practices of agricultural science, and in England the subject has even been openly debated in the House of Lords. There are, in fact, two schools of thought, two sets of *authorities.* One says that everything is well in hand, that fertility can be satisfactorily looked after by the accepted methods of orthodox soil science. The other says that fertility is fast running downhill, not merely in one country or area but in all parts of the world. And a great deal of this argument centers itself around the use of what are misleadingly known as artificial fertilizers.

The satisfied school—and by this I do not wish to suggest *complacent*—is mainly composed of research station scientists and many farmers. They contend that fertilizers will, if used properly,

maintain fertility. Their opponents, a minority school composed of some scientists, some writers, and some farmers, repeatedly say that we must abandon these chemical methods and instead must feed the soil with natural kinds of manures only.

It is always rather easier to arouse public interest with a campaign that attacks something orthodox than with a campaign that is only in favor of something already generally accepted. The antifertilizer argument, for this reason, tends to have rather a pull with the jury of ordinary opinion. Also, the ordinary man feels instinctively that dung must be best because deep-rooted in his consciousness is the knowledge that farmers for centuries relied upon dung and lime to feed the soil. Add to this the fact that he cannot quite forget that certain combines and companies make profits by producing and selling these chemicals, and remember that he has a skeptical department in his brain developed to some intensity by the recent course of economic events, and it is not difficult to realize that his reaction to the aggressive presentations of the antichemical argument has been that "there seems to be something in it."

It is, after all, a very serious and realistic matter. If chemicals are bad for soil fertility, let us cut them out. If they are not, let us discard doubts and make the fullest use of a modern scientific development. What are the facts? What is the truth? We are, however, not likely to see even a glimpse of this truth if we approach the problem with instincts, emotions, skepticisms, or fixed ideas. It is no effort of constructive thought, for example, to say that science has tampered with the laws of Nature and we must stop this nonsense and return to Nature. A capital "N" belongs to Nature by tradition, but we need not automatically assume her to be our infallible mistress, nor immobilize our brains when we seek to court her. Nature is as often our enemy as she is our friend. Similarly, in this argument about fertilizers, it is mere press-button thinking to assert that, as usual, *they knew better in the good old days*. As a nation we are overfond of these yearning compliments to a rosy past and we inquire too seldom into

the actual details of the roses. History is taught in our schools with an accent upon the glorious and with a discreet suppression of the historical common man's social realities.

There is only one way to get at the truth, or at any rate to make the attempt, and that is by a well-disciplined survey of the relative facts, or of as many facts as are known. A man deciding which brand of cigarette to smoke may well reach a pleasing conclusion through instinct, whim, or chance, and without any major effort to consider real facts about the cigarettes themselves. The same man, called to a jury in a trial for murder, must by sheer weight of responsibility make a much bigger effort in balanced judgment. Whim, chance, instinct—these inadequate factors in opinion-formation must be cast aside. This fertilizer controversy must be treated in the same way; for if, as some have sensationally suggested, fertilizers are poisoning the soil, then we are being poisoned too.

In all incomplete fields of inquiry, the element of human bias tends to intrude. I must, therefore, give warning about the kind of bias I am likely to have; because that bias must assert itself inasmuch as I have to decide in writing this book, what to put in as important and what to leave out as unimportant. I shall do my best to present facts and deductions without prejudice, but I am a chemist and moreover a chemist concerned in the production of these chemicals that are rightly or wrongly used as fertilizers. A deduction that satisfies my *chemically* trained kind of skepticism might not equally satisfy somebody else with a *biologically* trained skepticism. So I suggest that my probable bias should be borne in mind by the reader, and here and there a certain amount of discounting might be wise. Where I seem to become an advocate instead of a mere witness, then I must be read with very close caution. In many kinds of laboratory measurements scientists allow for the standard error of the method they employ; they take no notice of a 2 per cent difference between comparative measurements if the accuracy of the instruments used is such that errors up to 1 per cent either way are possible. The human being as a scientific instrument is subject

to a much wider error than 1 per cent even in the most well regulated and inhuman cases.

Indeed, we begin with a demonstration of this personal bias in operation. It is my opinion, that is to say, a matter of my bias, that no one can fairly judge the case *against* chemical fertilizers unless he first understands the case *for* them, until he appreciates how and why it came to be believed that certain chemicals could make two blades of grass grow where only one would grow without them. I am therefore putting the argument for fertilizers in front of the argument against them. However, in any comparison, whether it be that of a cricket match or the cases for *crown* and *accused* in a trial, one side or the other has to *go in* first; and, if fertilizers gain any advantage through being given priority, then a discount must be made for that.

And so to soil. The use of fertilizers is but one of many actions in the care of the soil, and alone the application of fertilizers can achieve little or nothing. Fertility is affected by the nature of the soil itself, by the way it is physically handled, by drainage, by weather, by sequence of crops, and so on. Good husbandry is the total effect of dealing with all these factors. We must confine ourselves as much as possible to the aspect of things that are added to the soil to make it or to keep it productive, and we must assume that these other variables are more or less satisfactory. The constitution of the soil in terms of plant foods, the way this changes, what can be done to prevent adverse change and to encourage or initiate favorable change—these are the matters we must mainly consider. Other influences upon crop production, such as cultivation technique, must be *sub judice* to a large extent.

On the whole, the case for fertilizers has been poorly stated from the point of view of the ordinary man. The research work accounts have been confined to appropriate scientific journals and to textbooks for students. Information about fertilizers has not been lacking in farming and gardening literature, but it has been usually of the rather dictatorial do-this-and-this-will-happen variety. The intelligent layman gleans that nitrogen, phosphorus,

and potash are plant foods and that fertilizers contain them, and that all he has to do is to remember how many ounces per square yard or hundredweights per acre to apply. And the soundness of this rule of thumb information has to be assumed. At this point let a quotation from the foreword to Professor Hogben's *Science for the Citizen* interrupt:

"The key to the eloquent literature which the pen of Faraday and Huxley produced is their firm faith in the educability of mankind. Because I share that faith I have not asked the reader to take any reasoning on trust. . . ."

The ordinary man, particularly if he happens to have some direct responsibility to the soil, as landowner or farmer or commercial grower or just keen amateur gardener, should not be merely told what to do, he should be told *why* in a language that he can understand. This is acutely true about the scientific work upon which the case for chemical fertilizers rests. For, when the non-scientist comes up against the antifertilizer argument, very clearly expressed in his own kind of terms, he cannot but be convinced. His knowledge of the evidence for fertilizers is scrappy. It has been withheld from him—science has assumed that all he needed was the practical *dope*. Of course, this is exaggerated. There have been books and articles that have attempted successfully to explain the reasoning behind fertilizers. It is not an unsuitable subject for popular science; at least, many other fields of modern science are far more complicated, less accountable in terms of common sense. However, these attempts have not been sufficiently numerous. Many quite considerable users of fertilizers know nothing more about them than rule of thumb recipes. Scientists in laboratories have made up the recipes; that is good enough. However, is it good enough when along come other clever people who say that the scientific story is unsound? It is the ordinary man who must judge, it is the ordinary man who will in most cases decide whether to run colossal compost heaps or to remain content with normal supplies of organic manures and supplement these with chemicals. How can he judge without knowing the reasoning behind *both* sides of the case?

The modern fertilizer developments started with Liebig, a German chemist, in 1840. Liebig analyzed the ashes of burnt plants and drew attention to the regular presence of certain mineral elements; he argued that these must have been drawn from the soil, and that what is called soil fertility must depend upon the presence of these elements in the soil. He went on to suggest that fertility could be maintained or improved by the addition of these elements in suitable forms to the soil. This, for better or worse, was the start. At about the same time, Lawes, a young Englishman with an estate at Rothamsted and a taste for chemical inquiry, began to experiment on his own land by adding to the soil chemical materials which would provide these nutrient elements. In 1843, encouraged by his own progress, he employed a professional chemist, Dr. Gilbert, to handle the chemical aspects of his experiments. It will not really help us much to follow the fertilizer story through in historical sequence because at various times some of the ideas have had to change, and it will only be confusing to discuss where Liebig was wrong and where Lawes was wrong. The historical fact is, however, that Lawes was able to show increased crops through the use of fertilizers, and he went about the business of demonstration very scientifically, comparing the growth of crops on similar soils with and without these chemical applications. Convinced that he was in on the ground floor of new and important developments, Lawes started a factory to manufacture fertilizers.

Though there was considerable interest in this new method of food production, stimulated possibly by the loss of so much food by the potato blight attacks of 1845 and 1846, fertilizers had to fight against stolid resistance. *Muck,* after all, was produced by Nature and crops were grown by Nature! However, Lawes and Gilbert went on with their experimenting, perhaps little realizing that some of the tests they were conducting would still be continuing over a hundred years later on precisely the same plots of ground. They measured the amounts of fertilizer they applied, they measured the crop yields they obtained, and they took samples for analysis in the laboratory. There was nothing hap-

hazard about their methods of investigation; they were not content with the kind of proof that was solely in the taste of the pudding. They wanted to know *why* the pudding's taste was satisfactory.

The Rothamsted estate was eventually turned into a research station and the work of Lawes and Gilbert was carried on after their deaths by others, notably by the late Sir Daniel Hall and by Sir John Russell, who recently retired from the directorship of the station. For over a hundred years the original Lawes estate has been the center of fertilizer research, and during that time many other similar organizations in other parts of the world have joined in. There is, therefore, an enormous mass of data about the results of using fertilizers of all kinds. The sets of conditions which have been investigated are almost innumerable. So that the scientific case does not rest upon some highly theoretical basis or upon a few experiments; on the contrary, it is founded upon a wide range of experimental work.

However, the development in the use of fertilizers has not followed from all this research work in quite the obvious way one might think. In the twentieth century other pressures than the persuasive pressure of scientific results have popularized the chemical fertilizers. In many areas specialized arable farming has displaced diversified farming, so that large acreages of crops are grown without much rotation and without much complementary production of farmyard manure. The specialist arable farmers reached out to the chemicals not as additions to manure but as necessitous substitutes for manure. To some extent the amounts of animal manures have declined as a result of the displacement of horses by motors; city stables no longer offer extra humus supplies to farms on the city outskirts. Although on figures alone it can be shown that the total number of livestock has not declined, it must be remembered that in modern times many of the animals are allowed much shorter lives. All these factors, external to the basic argument whether fertilizers are good for the soil or bad, have led many to use chemicals not from conviction via science but from compulsion by dire necessity. In short,

fertilizers often won their chance on a farm without much reference to the real issue but through circumstances causing a shortage of manure. So that today in many opinions fertilizers are simply a *substitute for muck.* A civil servant of the higher grade and permanent variety during a wartime visit to a fertilizer works said: "Of course, this sort of stuff won't be much needed after the war, will it?" A member of parliament, writing in a local paper in her constituency, suggested that farmers were longing for the return of peace when they could get back to using dung instead of chemicals.

This attitude springs from the circumstances by which fertilizers have impinged upon farming. However, having no relation to the real issue, it is a fallacious attitude based upon the erroneous view that chemicals can replace manures. They cannot do this, they are complements to manures, additions not substitutes. This must be understood from the start, hung on to throughout all the arguing of this book like a lifebelt.

We can best clear this issue up by considering the main features of a fertile soil, and seeing just which of those features chemicals can help and which of them manures can help. For, if there is some difference in function, then one clearly cannot properly take the other's place or even act as a passable understudy.

Three changes are constantly taking place in the soil. First, acidity is tending to develop. Second, humus—the soil's content of decomposed organic matter—is being used up. Third, certain elements essential to plant growth are being removed by the harvested crops or removed in other ways. These statements are not opinions, they are established facts, recorded and proved by thousands of experiments. Neither the fertilizer nor the anti-fertilizer school of thought will quarrel about them; the quarreling begins only when we start talking about methods of compensating for these changes. A soil's continued ability to produce good crops depends not upon preventing these changes but upon making up for them. The changes are part of the cost of growing a crop, the inevitable loss that must accompany a gain; just as the reduction of a bank balance is the inevitable consequence of

meeting bills for existence. The soil has this difference from a bank: it has no head office to arrange overdrafts even for the most deserving clients. The credit system belongs to man, not to Nature.

First and briefly, this question of acidity. Why should soils tend to get acid? It seems, after all, a very poor arrangement on the part of Nature. We must not forget that the human species is only a minor item on the agenda of Nature, if indeed Nature has any agenda at all. We are far too conceited. It may not suit the kind of plant life we want for our food that the soil gets acid, but Nature has plenty of other kinds of vegetation that can thrive upon acid conditions. We happen mainly to feed upon those that, on the whole, dislike acidity. And even if, with ever increasing acidity, vegetation ultimately ceases, we have no reason to suppose that Nature cares very much about that. Nature has created deserts in many parts of the world and it is a self-interested view for us to think of Nature only in terms of lush green growth or majestic forests. If we are to go on producing enough food for ourselves from the soil we must continually correct Nature's acid tendency which conflicts with our necessities.

The actual fact that soils do get acid is easy enough to determine. Even the most skeptical reader will take it on trust that chemists can measure this kind of change by analyzing soil samples. The accurate measurement of soil acidity today is in terms of what is called the pH, which is a mathematical expression of *state of acidity* in actual operation, whereas measurement of the amount of acid actually present by neutralization with a known amount of alkali expresses both the actual degree of acidity and the potential degree (for some of the acid in the soil may not be actively functioning as an acid until in the test an alkali is added). We need not go into this pH business any more than that. It is more important to look at the causes of acid development.

These causes are several. The decomposition of organic matter produces a weak acid known as *humic acid,* which is a general description covering a multitude of complex substances not yet fully understood. Also, the alkaline parts of mineral salts in the

soil are more frequently removed than the acid parts. The rain will wash the alkaline parts out more readily, and when plants take up mineral nutrients it is rather more often the alkaline part that is thus broken up than the acid part. These acid residues join themselves on to any free alkali (such as lime) in the soil, and so the free alkaline nature of the soil is steadily reduced. The shifting balance is always in favor of acidity.

The time-honored remedy is the direct application of lime. Indeed, liming is older than Christianity. But lime is generally not regarded as a fertilizer. Calcium is needed as an essential element to plant growth, but calcium is generally abundantly present in the soil without lime addition, and we add lime not so much for any increase in calcium supply but because it is the cheapest and best distributed material with which to neutralize acidity.

Second comes the humus problem. If the word *humus* is not included in the vocabulary of Basic English, it will be very difficult to write about soil fertility in this dehydrated language. Humus is a word that was invented before the days of Liebig to cover up a large number of complexities that could not be simplified, and the word remains because the situation also remains. I do not mean by this that our knowledge of humus and its properties has not advanced since the early nineteenth century; it most certainly has. S. A. Waksman's treatise on humus runs to over five hundred pages. However, we are still very much in the dark about the precise composition of humus and exactly why it is so important; much of our knowledge is of the observation variety rather than of the interpretation variety. However, evidence that comes from observing effects must not be rated lower in value than evidence that can explain the effects. There is often a tendency to do this. The fact that can be fully understood presumably being more attractive and memorable than the fact that can merely be recorded. To take up again the analogy of the trial for murder: if a witness is produced who saw the accused stick a knife into the victim, that evidence, provided the witness is reliable, outweighs all the circumstantial evidence that

tries to show why the accused had reason to commit the murder or how he had the opportunity and so on.

Humus is the dark brown or black decomposed organic matter invariably noticeable in what are called rich soils. Farmyard manure, stable manure, vegetable waste matter, these in their fresh forms are not humus but rather the raw materials that can be turned into humus. By far the simplest way to interpret humus is to list the things it can do. Its properties—from the point of view of soil fertility—can be divided into three classes; mechanical or physical, biological, and chemical.

The physical or mechanical effects are as follows. It can bind together a light, crumbling soil, but it can also make a sticky, heavy soil more friable. The erosion disasters in the United States, in which thousands of crop-producing acres became a desert or *dust-bowl,* are now generally admitted to have been caused by humus deficiency. The soils were originally very rich; they were farmed without attention to humus replacement—the top-soils became more and more friable, crumbled into dry dust; then, once a certain level in deterioration was reached, nothing could save the soils from being swept away by rough weather. Humus keeps the soil particles apart and so keeps air moving through the soil. It holds water better than soil so that plants in a humus-rich soil are less affected by drought conditions. Sir John Russell has reported that plots at Rothamsted regularly treated with farmyard manures contain 3 to 4 per cent more water than plots under similar cropping conditions but which receive non-humus containing manures. And, of course, every gardener knows how much better are his moisture-needing summer crops like beans, peas, tomatoes, marrows, etc., if rotted organic matter is trenched in underneath them. A minor physical effect comes from its color, for by tending to darken the soil it increases the absorption capacity of the soil for warm sun rays and thus can keep the soil temperature a little higher.

Its biological properties are vital. It increases the activities of so many organisms whose work is a favorable factor to soil fertility. From the earthworm to the invisible earth bacteria, the

e soil population is stimulated by the presence of is is an important matter that we shall have to con- .. much more detail later. For the moment let it be left at that. Chemically, humus—or at any rate the manures that contain humus—will contain supplies of the elements of plant-growth. This is obvious for the manures have been produced by the *rotting* of plant material—whether a cow has eaten, digested, and expelled grass or mangolds or whether waste green material has been directly composted in a heap. At this preliminary and general stage, we need not go into the question of how much of the original minerals, etc., taken from the soil by the plants will still remain in the humus-type manures which are later put back into the soil, but clearly the manures will have some definite value of this kind. Also, in this plant-food element department of soil fertility, humus plays an indirect role; for it can increase the soil's capacity for retaining soluble (and therefore active) kinds of these plant foods. As we shall see later, there is always a tendency for immediate fertility in soils to be lost through the soil's inability to hold all its active plant-food supply indefinitely. So that the help of humus in compensating for this adverse factor is important.

How can the humus content of the soil be kept up? By the digging or plowing in of animal manures—farmyard, stable, or sewage manures, by composting all organic wastes, by the deliberate growing of what are called *green manure* crops, e.g., mustard, for digging in, and by the digging in of all crop waste left after harvesting, e.g., stubble, mangold tops, and so on. When grassland is converted to arable land, as has happened so widely in wartime, the turned-in turf provides valuable humus as it slowly rots down in the soil. On the ley system of farming, with arable land and pasture land alternating, the grass crop is really an extreme case of green manuring even though it may be four or more years between seeding the grass and plowing it in.

It will be noted that the application of fertilizers is not given here as a *direct* method of humus provision; but the application of natural manures is. This is a fundamental difference in the

functions of these two kinds of fertility makers, and I personally would suggest that a very high percentage of the argument about fertilizers is due to failure to appreciate this difference. This does not mean that the chemical fertilizer cannot *indirectly* provide humus, for it can and does very frequently in farming practice. The green manure crop being increased in yield by the use of fertilizers, thus producing more humus when it is dug in. Mr. Raymond Bush, the well-known fruit cultivation authority (and incidentally an authority who is often very dubious about the benefits of chemicals) reports in one of his excellent Penguin books on fruit-growing that he applies sulfate of ammonia to nettles growing in a rough paddock so that he obtains more nettle growth to cut down and add to his compost heap. Note that the humus is not in the bag of fertilizer.

The difference between manures and fertilizers is confused by the fact that the manures contain not only humus but also some supplies of the fertility elements. In this latter sense, therefore, they overlap the function of fertilizers. We must neither exaggerate the value of this overlap, nor underestimate it. Important questions affecting the whole argument about fertilizers are: (1) how much *chemical* plant food do these natural manures provide, (2) how much natural manure of all kinds is, or can be made, available, (3) how much plant food must be added to the soil to maintain fertility at the level necessary for our requirements? It cannot be decided that natural manures can maintain a required standard of fertility without the help of fertilizers until we have drawn up a budget from answers to these questions. It is not enough to make a *qualitative* statement that natural manures can supply all that plants need; the problem is also *quantitative*. A simple explanation will make this clear. Gardeners (lucky ones) often say that they don't need fertilizers because they can get farmyard or stable manure. They may or may not be right—it depends entirely upon *how much* manure they get and how much plant food their rate of cropping needs.

It is the chemical plant foods with which fertilizers are more concerned. Liebig made the point that any element found by

analysis in the composition of a healthy plant was *ipso facto* an element necessary to its proper growth.

The elements found in plants generally are: carbon, nitrogen, hydrogen, oxygen, phosphorus, potassium, calcium, magnesium, sulfur, iron, manganese, chlorine, boron. Even this is not a complete list but it contains the main ones and some minor ones. It used to be thought that carbon, the predominant element in plant structure, was derived from the soil, but it was established by experiment that plants breathed in carbon dioxide from the air and exhaled oxygen, thus showing themselves to be inversely different from the human species. This process, known as photosynthesis because it is dependent upon light, is not a theory, but an experimentally determined fact, and simple proof of it can be found in any elementary textbook on botany and indeed in many general chemistry books. The plant obtains carbon, its main building raw material, from the carbon dioxide in the air. Oxygen is also obtained from the air and hydrogen (and perhaps some oxygen too) from water. Liebig thought that the nitrogen also came from the air, but he was wrong in supposing this, for we now know that atmospheric nitrogen, which is exceedingly unwilling to combine with anything, can be fed upon only by certain special bacteria. The nitrogen—with these exceptions—comes from the soil, as do all the other elements on the list.

Now, of these elements there are three important ones that the soil itself does not seem able to supply sufficiently *for our cropping needs:* nitrogen, phosphorus, and potassium. Each harvested crop takes away supplies of these elements that have come from the soil and, after a time, these losses reduce the soil's ability to go on feeding crops. By sampling and analysis it is a simple matter for a chemist to measure just how much of these elements is removed, say, per acre by a crop. Thus, a good crop of potatoes might take from the soil about 150 pounds of potash (oxide of potassium) per acre. What happens to this 150 pounds? The potatoes are eaten, digested, expelled from the human system into the sewage system. In a modern city this usually means that the sewage is conducted into a river or sea as quietly and unobtru-

sively as possible. That part of the potash in the discarded peelings may go on to a compost heap or be fed to pigs or poultry in which case a fraction of the potash will eventually find its way back to the soil. In sewage disposal, most of the potash is lost completely. Admitted, there is some sewage reclamation carried on, but it must be remembered that sewage in modern sanitation is heavily diluted with water and this means that the active plant food, the kind that can dissolve in water, must pass into the liquid fraction of sewage. It is this liquid fraction that is discarded in most systems; the sludges that are reclaimed at some works are composed of the solid, insoluble parts of sewage. These will be valuable humus suppliers but they will not be high in plant food content. There is, therefore, continuous loss. In less civilized countries, or perhaps it is fairer to say less industrialized countries, the sewage is disposed of by putting it directly back on to and into the soil.

In cattle farming, the nitrogen, phosphorus, and potash consumed when the cattle eat grass or fodder crops returns to the farm as manure—but even here there is loss, for the cattle retain some of the plant food for their own growth and health-maintenance; some of this is eventually disposed of as sewage (after the meat has been eaten or the milk drunk) and some comes back to the soil as organic fertilizers when the slaughterhouse wastage is turned into such products as dried blood, bone meal, steamed bone flour, and so on. There is also a substantial loss in the processes of making manure. However, the main point is that, even with cattle farming, there is loss.

How resilient is the soil? How much can it make up for these losses from its own stocks? How can we handle the soil so as to increase this resilience or to compensate for the losses? The idea of fertilizers is, of course, the idea of adding external supplies of nitrogen, phosphorus, and potash to balance the budget. The idea is supported by the fact that, when this is done, crops show striking responses.

All this seems more than enough for a general preview of the situation. The picture has indeed been oversimplified. We must

in the chapters that follow examine most of these points in detail. This preliminary review of soil needs has been intended simply to show the general picture, to show how the details interconnect.

CHAPTER II

THE CHEMICAL TRIO—NPK *

"If science had not proved that plants grow from this slender nourishment I think no one would have guessed it; plants are so very much more substantial than their food. An oak tree rears its branches to the height of a house and carries all its great sail area of leaves securely.... Yet this, they say, is made of a salt solution so weak that it has no taste of salt, and of the tenuous gases of the air." MICHAEL GRAHAM, *Soil and Sense.*

THOSE WHO oppose fertilizers are apt to speak rather contemptuously of the NPK mentality, the implication being, of course, that it is scientifically a parochial or suburban mentality. The fact that the first chapter in this book to deal with any fertility aspect in detail is about the NPK and other chemical nutrients must not be regarded as a deliberate snub for humus or even for lime. It is simply a precedence decided by my bias in thinking that the case for fertilizers must be considered before the argument about them can be fully understood.

The principal chemical nutrients are *nitrogen, phosphorus,* and *potassium.* The others that plants derive from the soil are regarded comparatively as minor elements. Before restricting the discussion to the principal trio, it would be wise to decide just what is meant by this division into *major* and *minor.* For example, calcium is badly needed by plants and is just as major a nutrient as the others; but its supply from the soil does not present a major problem. In most soils there is enough calcium always present for calcium deficiency not to arise. Therefore cal-

* N = Nitrogen calculated as N_1.
P = Phosphorus calculated as P_2O_5, called phosphoric acid.
K = Potassium calculated as K_2O, called potash.

cium is not regarded with that same awe as nitrogen, phosphorus, and potassium. The other elements concerned in plant growth such as iron, sulfur, boron, manganese, magnesium are usually called minor elements or trace elements because (a) they are needed in very small quantities only, or (b) as with calcium, the amounts required can usually be supplied adequately from the soil's own resources. However, their importance to the plant is *not* minor. A key may weigh only an ounce or so but its importance to a locked door weighing many pounds and shutting up a house weighing several tons is hardly minor and out of all proportion to its relative weight. These so-called trace elements are rather like the key. Their functions must take place or the functions performed by other, the so-called major nutrients are handicapped or prevented. We can return in a later part of this book to these lesser nutrients, but for the moment let these indications serve to put the so-called major and minor kinds of plant foods into perspective. *Trace* nutrients or *trace* elements are terms much less misleading than the frequently used one of *minor* elements. In the equilibria of the natural and living world trifles and little things are frequently of great account, perhaps rather more so than in our man-made equilibria of civilization where so often we pay homage to the big and weighty and ignore or despise the quiet and tiny.

It must not be thought, by the way, that soils invariably supply these trace nutrients as and when required. To this general rule there are exceptions, and deficiencies do occur, often with severe results. However, fertilizer practice, so far and on the whole, has not included this problem largely because excesses of the trace elements are often toxic; just as small doses of strychnine and arsenic are medicinal and yet larger doses will lead to Scotland Yard investigations and Sunday paper sensations. Trace nutrient deficiencies have to be tackled as special cases when they turn up and the advice of specialists sought. Boron deficiency will cause beet to rot internally; magnesium deficiency will prevent green coloration of foliage and tissue because the green pigment, chlorophyll, contains magnesium as a component of its molecule.

Before tackling the main trio, the terminology used in talking about these matters must be explained, for fertilizer science and fertilizer commerce have developed their own semiscientific jargon.

We talk about nitrogen as nitrogen, which is of course an eminently sensible and obvious thing to do. However, we do not talk about phosphorus as phosphorus—we express the phosphorus value of soils and materials in terms of *phosphoric acid,* by which we mean one of the oxides of phosphorus: P_2O_5, phosphorus pentoxide, two atoms of phosphorus combined with five of oxygen. The odd thing is that this P_2O_5 is actually not phosphoric acid at all, but only the oxide that becomes the acid when it is further combined with water. In the jargon of fertilizers, phosphoric acid means this P_2O_5, and chemical accuracy is disregarded very much as Queen Victoria disregarded the possibilities of defeat. We also do not talk about potassium, always preferring to measure the amount of this element in terms of its oxide, potash or K_2O—two atoms of potassium coupled with one of oxygen.

When soils or fertilizers or manures are analyzed for their plant-food content (in the chemical sense) the results are expressed as so much percentage by weight of (1) nitrogen, N; (2) phosphoric acid, P_2O_5; (3) potash, K_2O. The phosphoric acid is divided into two kinds, soluble and insoluble, but that particular aspect of the matter can wait. Nobody should think there is a catch in the adoption of this method of expression. So much phosphorus is always equivalent to so much phosphoric acid, for two atoms of phosphorus will always be combined with five of oxygen and there is an unalterable weight ratio between the element and the oxide. Thus 1 unit of phosphorus is always equivalent to approximately 2.3 units of P_2O_5. And similarly 1 unit of potassium is always equivalent to approximately 1.2 units of potash.

Another point to get settled is the fact that no soil or fertilizer ever contains any phosphoric acid or potash at all. They actually contain other *compounds* of phosphorus and potassium. I hope

this is not seeming too illogical to the non-chemically minded. For it is really quite logical. We may have supplies of these elements in many different kinds of compounds, and we must choose some common yardstick by which to measure them. Therefore, the results of analyses are always worked out in the convenient equivalent weights of phosphoric acid and potash. P_2O_5 and K_2O never turn up in a free form because they are very non-isolationist kinds of substances. If left alone they will combine with water very rapidly, even stealing it from other substances as well as from the moisture of the air. A commercial fertilizer contains, e.g., muriate of potash and is today generally described as 60 per cent potash. This fertilizer is a commercial grade of potassium chloride and contains no K_2O. The statement 60 per cent potash means that, if the potassium present there as chloride were turned into the oxide, the potash, 60 per cent of the weight of the fertilizer were potash.

Nitrogen used to be expressed not as nitrogen but as the equivalent amount of ammonia, NH_3, one atom of nitrogen combined with three of hydrogen, and phosphorus was formerly always expressed as the equivalent weight of calcium phosphate, $Ca_3(PO_4)_2$. The weights of ammonia and calcium phosphate are higher than those of nitrogen and phosphoric acid. One unit of nitrogen corresponds to 1.2 units of ammonia and one unit of phosphoric acid to 2.2 of calcium phosphate (again approximately to the first decimal place). However, this older terminology must still be borne in mind because some of the older books on farming and gardening use it; and some fertilizer suppliers still seem to regret rather nostalgically the Fertilizers Act that brought in the new system, for not content with the new figures they describe their products in terms of the old ones as well. No buyer should fall for the idea that a fertilizer whose analysis is given for nitrogen *and* for ammonia contains both. The seller is simply saying the same thing twice. Similarly, for phosphoric acid and phosphate values—though two figures may be given, both mean exactly the same amount of phosphorus. I cannot help

feeling that if the original system had expressed the fertilizer values in *smaller* figures, these old-fashioned fertilizer sellers would have caught up far more quickly with the times.

This seems enough about terminology for the time being. In the rest of this book the three main nutrients will be referred to as nitrogen, phosphoric acid, and potash, and, when all three are referred to as a collective trio, the shorthand term, NPK, will be used. Thus an NPK fertilizer will be one that supplies amounts of all three nutrients. Sometimes, however, the older term *phosphates* will slip in, because it is rather more euphonious than phosphoric acid, but no distinction between phosphoric acid and phosphates, unless specifically stated, will be implied.

Why is it that NPK supplies are so vital to plant growth? The question is even more pressing when it is realized that, though these are major nutrients, the amounts actually held in the plant's composition are relatively small. Thus the root parts of mangolds contain about 0.2 per cent nitrogen, 0.06 per cent phosphoric acid, and 0.4 per cent potash. These are certainly not large percentages for so essential substances.

The functions of nitrogen and phosphorus and potassium cannot be discussed collectively, because they are different. Yet, in looking at them separately, the fact must not be lost sight of that the functions are coordinated, are interdependent. They are affecting the same entity, the plant, and the balanced development of the plant is brought about by the interlocking of these functions. A great deal of the misuse of fertilizers is due to applying unbalanced amounts of nutrients so that one function is overperformed while the other drags along in a dreary struggle because of a deficiency.

It seems that plants absorb nutrients from the soil via their roots and root-hairs. Some biologists consider that the total feeding process also depends upon a fungus parasite attached to the roots, the fungus, called mycorrhiza, acting as a bridge between nutrient supply and plant cells. Even those who believe this to be true do not say that it is true for all plants, for some are non-

mycorrhizal. At any rate, plants can be grown without soil at all, simply with their roots suspended in water solutions of various nutrients. This is now being developed as a practical method of cropping, particularly in America, and, when practiced as a deliberate method of food or flower production, it is called hydroponics. It has been known as a special laboratory method of investigation for many years. By this method the various effects of plant-feeding elements have been separated and studied, for you can put what you like into pure water to feed a plant, whereas it is far more difficult to control what is in a soil even though you may control what is added artificially.

By observing what happens when various plants grow in solutions containing various amounts of NPK and minor chemicals, botanists and chemists have come to the following conclusions. They can be regarded as generally acceptable conclusions for they have been derived from hundreds of observations in hundreds of different cases, and whenever and wherever these kinds of experiments are repeated the same kinds of results turn up.

Nitrogen is the food that plants need to grow leaf and stem. Plenty of nitrogen means a fast-growing plant with an extensive leaf area and size of stem. Deficiency of nitrogen means a stunted, slow-growing plant. Generally the leaves are dark green when nitrogen is sufficiently present, but yellowish-green and even yellow where nitrogen is deficient.

Phosphoric acid stimulates seedling development, the formation of a good root system, and later on in the life of a plant it encourages early ripening.

Potash stimulates the efficiency with which the leaf part of the plant inhales carbon dioxide and holds the carbon to turn it into carbohydrates (sugars, starches). It is the predominant stimulator of ripening. It seems to be connected with the health and vigor of plants, for a good potash supply has frequently been observed to enable a plant to withstand pest attacks or other adverse factors.

These are the main observations drawn from studies of cases in which sufficiencies and deficiencies of nitrogen, phosphorus

and potassium have been deliberately devised. We can see how they interlock in Nature by adding their functions up. There may be a good store of nitrogen for a plant to feed upon, but if the phosphoric acid store is very low the plant's root development will be poor; and because of this the plant's ability to absorb the nitrogen will be restricted. Similarly, nitrogen again may be plentiful and phosphoric acid also plentiful so that large roots and leaves are formed, but, if potash is absent or very deficient, these leaves will not function efficiently in *fixing* carbon, and the carbohydrate-making part of the plant will be slow and inadequate. If nitrogen is in short supply, the plant will not be able to take much advantage of adequate phosphoric acid and potash rations since the root system will have little nitrogen to absorb and the leaves will be stunted, however efficient the potash may make them in performing the carbon in-take function. These functions clearly hang together for satisfactory plant development. No one should handle fertilizers unless he realizes this. A plant forced by unbalanced feeding to grow in an unbalanced way is a deformity, and in this sense the misuse of fertilizers has undoubtedly given rise to unhealthy plants. For deformed plants will resist disease much less than balanced plants.

The important thing to realize is that these are not theories or speculations, but experimentally observed facts. No one has shaken a few test tubes or covered sheets of paper with mathematical symbols and then announced that nitrogen controls leaf growth and phosphoric acid controls root growth. Circumstantial evidence has not had to come into the picture at all. These things happen when plants are fed with these nutrients. And they do not happen, or they happen much less, when the nutrients are absent or scarce.

So far as interpretative theory is concerned, we are still not very well informed as to how nitrogen dominates leaf growth, how phosphoric acid dominates root development, or how potash supervises the carbon handling, etc. So, in the general acceptance of these main functions of nitrogen, phosphorus and potassium, it can quite definitely be said that there has been precious little

jumping to conclusions or deducing or any of the other gambits of theorizing. Science has theorized neither before nor after the event.

It should not be thought that the balanced requirements of all plants are alike. Indeed, for different types of plant, the balance is very different. For example, a lettuce is predominantly a leaf plant (from our limited human food angle) and consequently its main nutrient need will be nitrogen, with some phosphoric acid to stimulate the root development and some potash to keep it vigorous. With the tomato plant we do not want a lot of leaf, only enough to ensure speedy early development and sufficient carbon fixation to make the carbohydrate (sugar) for the fruit. If we give a tomato plant too much nitrogen, it produces so much leaf and stem that it is slow in getting on with the job of flowering and fruiting, but phosphoric acid for root formation and ripening assistance, and potash for sugar formation and ripening and vigor are essential. In this case potash is obviously the most vital nutrient of all. So the balance of food needs will vary considerably from one type of plant to another, particularly when we aim at cultivating each plant so that its development most fits in with the needs of our stomachs. The point cannot be missed that the deliberate guiding of a plant toward our designs for it may well be partly achieved by choosing for it a balance of NPK that will tend to produce the parts of the plant we want, though such an imposing of man's will upon the plant should not be pressed too far.

It should be added that although these NPK functions were observed by separating them in water culture experiments, the findings of these experiments have been confirmed over and over again in the more normal soil-growth. Nobody need feel skeptical and observe (as would be quite fair in absence of further confirmation) that what happens in the abnormal system of growth in water is no certain indication of what happens when plants grow with their roots embedded in earth.

The effects of various chemical nutrients on plants are also isolated by growing the plants in sand, which is an inert medium,

the nutrients being given by applying them in aqueous solutions. Sand culture is often preferred since the sand holds up the plant in a mechanical sense just as soil would. The principle of these experiments is exactly the same as in water cultures.

An experiment from the Long Ashton Research Station, quoted by Professor Wallace in his book, *The Diagnosis of Mineral Deficiencies in Plants,* illustrates most effectively how scientists have investigated nutrient effects upon plant growth by these sand or water culture tests. This series of experiments was conducted with potato plants, and the effect of omitting certain nutrients upon the size of the tubers was measured.

Treatment	*Average Weight per Tuber grams*
Complete nutrient	60.1
Nitrogen omitted	26.8
Phosphorus omitted	30.8
Calcium omitted	2.4
Magnesium omitted	15.6
Potash omitted	16.9

The startling effects of omitting calcium and magnesium—which are not in our major NPK trio—shows clearly that no element needed by a plant is necessarily needed only in a minor sense.

However, because the NPK nutrients definitely perform these major tasks in controlling plant-development, it must not be assumed that a case for fertilizers is thereby directly established. We have to see whether these functions actually are better performed when fertilizers are added to the soil, and to consider whether the soil can by itself, or by some other method of help, do as well, interpreting that *as well* in the sense of a *long-term* basis. We have made no case for fertilizers yet. We have only indicated a reason for believing that fertilizers—materials supplying nitrogen, phosphorus and potassium—*might* stimulate crop production.

CHAPTER III

FIGURES

"In short, *to make an Authentic Experiment*, an *identity* of *Place, Time, Element*, and *Process*, must be strictly observed in every particular, excepting only the *Intended Difference* which constitutes the Experiment. Nor can the Experiment be *authentic*, if the Process be in any instance left to an *Agent;* it must be performed by the immediate hand, or under the immediate eye of the *Experimentalist*. . . ." WILLIAM MARSHALL from *Experiments and Observations Concerning Agriculture and the Weather*, 1779.

SINCE nitrogen and phosphorus and potassium are of such vital importance to the processes of plant-growth, what happens when additional supplies are added to those amounts already in the soil? If crops are improved by such additions, if the output per acre can be considerably increased, then it seems to be indicated that the actual supply of these nutrients from the soil itself is inadequate. Though, of course, the rider should be added here that a quantity increase in crop yield may not be an improvement at all if the crop quality has at the same time diminished. In this chapter, the major consideration will be *weight*—the increase in amount of crops obtained by the use of fertilizers. Until recently, the quality aspect of this matter has been largely taken for granted, and this is very understandable because there have been no obvious differences between crops grown with and without fertilizers. It has been critically suggested, however, that the nutritional value of crops grown with the help of chemicals is less than those grown on natural manures. At this stage it seems sufficient to mention this point and to leave consideration of it until the second part of the book, when the case against fertilizers is examined.

Farmers often check the fertilizers they are buying by leaving a small strip of their field untreated, and the subsequent differences between this strip and the rest of the field form a rough measure of the value of the fertilizer. This is practical enough, but it is hardly the precise method that scientific research requires. Visual observation and expression of the results in terms of *good* or *bad* or *moderate* and so on are altogether too vague, too dependent upon the personal standards of the observer. What is good enough for one man may well be quite ordinary for another. Fertilizer effects must be measured, must be expressed in reliable figures. It is in any case a general method of scientific investigation, almost the first stage in the tackling of any new problem, to attempt to describe the factors involved in figures related to each other on the same scale.

The actual figure for crop yield after using a fertilizer is of little value by itself. It becomes significant only if we have also the figure for crop-yield without the fertilizer, *but* with all the other affecting variables (weather, quality of seed, etc.) the same. A man may grow a crop of eight tons in 1943 without fertilizer; in 1944, persuaded to use a fertilizer or fertilizers, his crop on the same field may be ten tons. It is a very superficial judgment to assume that the fertilizer application has thus directly given a crop response of two tons, for other conditions that affect crop-yields have varied as well. The weather will not have been the same in the two seasons, the quality of seed will probably have varied to some extent, and the soil in 1944 may be initially poorer than it was in 1943 by the amounts of nutrients removed in the cropping of 1943. So that, although the two tons increase in crop somewhat powerfully suggests fertilizer efficiency, it is by no means a precise measurement. Yet most opinions upon the effects of fertilizers are formed by considerations of this kind; indeed, actual crop-weight effects are often not known, observation being only in these terms of *good* and *bad*. In this attempt to decide whether fertilizers should or should not be used in general agriculture, we should be very cautious—to say the least—in accepting any evidence that is not given *in numerical terms* and where the

conditions of measurement have not been *precisely* described. Too often are we misled by the enthusiast who claims much better results with one particular fertilizer or manure. We must have from him a reliable figure for these *better results* and we must compare this figure with another figure expressing the results of not giving his favorite treatment to the same soil, the same crop, and under the same conditions.

The scientific method of overcoming these difficulties is the method of the long-term field test. Such tests have been described by Francis Bacon as far back as 1627, and an intriguing account of early researches of this kind will be found in a paper by G. E. Fussell in the 1935 Journal of the Royal Agricultural Society. The method, which is based upon common sense and not upon any specialized aspect of science, has often been used before the intervention of modern chemistry into soil and plant relationship. Nevertheless, it was in the hands of Lawes and Gilbert at Rothamsted that the plot-test method first expanded into a really discerning instrument of inquiry and deduction.

An area of ground, as uniform as possible in such matters as slope, drainage, type of soil, exposure to sun and wind, etc., is subdivided into equi-sized plots. These plots are treated in exactly the same way for soil handling, seed, cultivation, and so on; only their treatments with manure or fertilizer are *deliberately* varied. In such a test, Treatment A might be: *no manure or fertilizer,* Treatment B: *fertilizer supplying phosphorus and potassium, but no nitrogen,* Treatment C: *fertilizer supplying phosphorus and nitrogen, but no potassium,* and so on. Each treatment is usually carried out in triplicate to guard against the possibility of freak results through the operation of some accident or unexpected variable. The place for this kind of work is very clearly a special farm or research station,) and the people who conduct it should equally clearly be specially trained scientific workers. An eye upon the final market value of the test-crops, for example, would be fatal; for, if one plot looked like producing unsaleable vegetables, interference with this development would demolish the test even though it might improve the

bank balance of the crop owner. Enthusiasm for one soil treatment rather than another can equally be fatal, for this can almost unconsciously lead to a more loving care for those plots that embody the favored application.

At the end of the test, the various crop-yields are carefully measured, and the crop and the soil are sampled and analyzed. The differences between the various crops can then be ascribed to the differences in the treatments—*but* with reservations. The weather conditions may, despite their total constancy for all plots, have affected one treatment more than another; thus, very dry weather could handicap the beneficial effects of a heavy fertilizer application while relatively helping the effects of a manure application. Also, there is the question—how accurate can such measurements be? Crop differences may be only differences due to unavoidable errors in measurements. To guard against this weakness, scientists assess their own likely scale of error. By employing the methods of another science, the science of statistics, they are able to settle upon a figure for the *standard error* in their work. Thus, their crop-yield figures may have an error of 5 per cent either way, plus or minus. In that case, a crop yield of 100 units could truly be either 95 or 105—and crop differences that are less than 10 per cent different are *insignificant*. For those differences can be due just as much to the standard error as to the difference in fertilizer treatment. This is rather an oversimplified account of the statistical analysis of plot-test results, for the method involves a more complicated mathematical analysis than these obvious additions and subtractions. One of the leading opponents of fertilizer practice has fiercely criticized this introduction of statistical science, regarding it as some kind of mystical trickery to cook the figures. This critic has all the writer's sympathy, but it is not very fair or logical to criticize the method, because all that has happened is that another kind of science has been called in to distinguish with greater precision between results that are significant and results that are more or less meaningless.

To be of really solid value, *the tests must be long-term*. The

one season test is only an indication of truth, and it can be a misleading indication. For one thing, we gain a result that applies only to the weather conditions of that particular period. For another, we have not by any means neutralized the effect of the soil's own supply of nutrients. Though we have not yet considered in any detail the soil's capacity to supply NPK, it is obvious that all soils have some capacity to do this. In the first year of these plot treatments, we may well find that crop differences are far from sharp. A plot in which there is deliberately *no nitrogen added* might show very little initial effect from this omission, the soil itself producing sufficient nitrogen for reasonable plant development. Not until the second, third, or even later season might the soil's capacity to balance the unbalanced treatment fail to a marked degree. It depends upon just what the individual soil's nutrient supply capacity happens to be. Of course, soil will always be able to produce *some* nutrient supply however poor a view we may take of this capacity, but, after a time, this natural rate of supply will tend to be common, or more or less common, to all plots. It will be a fixed condition which can reasonably be cancelled out. This cannot be taken for granted in anything less than a long-term test. For this very good reason research stations carry on their tests season after season with the most commendable patience. Newspapers and the cinema so often paint a dazzling picture of scientific research, but how frequently the agricultural research worker must long for the varied and exciting life of a bank clerk. The results of 10 years' patient plot measurements can often be significantly summarized in a half-page of figures.

It may be objected that long-term tests of this kind are artificial. They involve the continuous growth of the same crop season after season upon the same soil. A deliberately unbalanced fertilizer treatment produces a deliberately unbalanced soil. Admittedly, many plots do become extreme cases of nutrient deficiency or excess as time goes on. However, the scientist is aiming at the isolation of the effects of certain treatments, and he is prepared to work back by deductions from his measurements of

these deliberate abnormalities. It is not so much the tests themselves that matter, but what is deduced from them. We can best decide whether or not this criticism is sound by looking at actual tests and deciding for ourselves whether the deductions drawn from them are reasonable or unreasonable.

Fairly obviously, the potential value of tests depends very much upon how they are initially planned. The most notable aspect of the early work of Lawes and Gilbert is the strategic vision with which they planned their first field tests; for many of these are still being continued today, season after season, on the same Rothamsted soil, on the very same units of soil! Some of the evidence for fertilizers can be drawn from experiments that have so far been conducted for over a century, that have been continuously carried on since the early eighteen-forties. We can take average crop yields over long periods during these tests. The Rothamsted contribution to modern science is one of the greatest world contributions ever made in any branch of logical investigation—yet, outside limited circles, it is comparatively unknown.

In 1876 another research station was started at Woburn, founded jointly by the Duke of Bedford and the Royal Agricultural Society. Here the work was directed by Dr. Voelcker until his death in 1884, when he was succeeded by his son. In 1926, the R.A.S. gave up the station and it became a sub-station under the management of Rothamsted. The Woburn station was important,. for the Rothamsted soil was heavy soil, and it was always a fair argument against their results that these were true only for heavy soils. However, Woburn soils were light, and when, after a time, the same trends that had been indicated at Rothamsted were shown at Woburn, the general scientific case was greatly strengthened.

Today we need not look to Rothamsted and Woburn alone. We can indeed afford to be skeptical and remain unconvinced by figures from any single center of investigation. There are research stations all over the world. To show what fertilizers can and cannot do, there is an enormous mass of reliable numerical

evidence. Judgment can be based upon thousands of tests. With the help of photography, we can see for ourselves the appearance effects of different treatments, we need not rely upon what this or that observer says. A book twenty times as long as this one could be filled with data and illustrations only.

Here are some test figures from Rothamsted. The crop is wheat. The period taken is 55 years, and the plots received the same treatments throughout this time. The yields given are the average yields over the 55 years.

Treatment	*Grain* *bushels*	*Straw* *hundredweights* *
No manure	12.9	10.5
NPK fertilizer	31.6	31.9
Fertilizer with phosphorus and potassium, no nitrogen	14.8	12.3
Fertilizer with nitrogen and phosphorus, no potassium	23.7	22.8
Fertilizer with nitrogen only	20.5	18.7

* Hundredweight (cwt.) = 112 pounds.

What do these figures seem to show? Most people would probably not say *seem to* here, but we do not want to be dogmatic. Figures are one thing, deductions another. Bias can enter the picture whenever we interpret even the most impressive numerical data. The casual deductions of Sherlock Holmes from trivial indications always seemed very convincing, but I have often wondered whether quite different and equally convincing deductions could not be drawn from the same facts.

The most striking difference is that between the no-manure treatment and the complete fertilizer treatment. The plot supplied with phosphorus and potassium is little different from the plot supplied with nothing, whereas the plots that have had nitrogen supplies show much better yields.

This shows how easy it is for fertilizers to earn a bad name. For just any fertilizer will not do. Leave out an adequate nitrogen

supply for wheat, and no matter how much phosphoric acid and potash is supplied the yield may not be very much better than if nothing had been added at all. Yet today many a small user will go into his local hardware or corn stores and ask, perhaps, for sulfate of ammonia (which supplies nitrogen only); and, this perhaps not being in stock, he is quite content to take away superphosphate instead.

To return to the Rothamsted figures. Note how well they can be explained by the ideas about the functions of nitrogen, phosphorus, and potassium discussed in the previous chapter. Without nitrogen, the wheat has been unable to grow adequate stem and leaf even though the other two nutrients have looked after root development and the utilization of carbon dioxide. With no potash the yield has been lower because the carbohydrate building system has not been efficient; without potash or phosphorus the root system as well has suffered, and the yield has been further reduced. So the deductions from one set of observations fit in with the observations of quite another set, which seems to make the combined matter much more certain to be true.

Here are some Woburn figures for barley grown on the same plots year after year. This time the figures have been expressed as averages for five-year periods, with yields in bushels of grain.

	Consecutive Five-Year Periods				
Treatment	*1st*	*2nd*	*3rd*	*4th*	*5th*
Unmanured	25.0	17.5	12.5	9.5	8.0
Nitrogen only	40.4	30.9	23.7	15.2	11.3
Nitrogen plus phosphorus and potassium	46.0	41.1	35.3	19.7	16.5
Double dose of nitrogen plus phosphorus and potassium	53.3	45.3	42.9	25.4	20.0

These figures show a continuous falling off in yield, due to the continuous treatment and cropping on the same plots, the effects of which show up much more readily on light soil than on heavy

soil. This set of figures may indeed raise the cry that here is proved very convincingly the argument against fertilizers that their effect cannot be maintained. We shall deal with this aspect more fully in a later chapter, but for the time being it must be insisted that these continuous treatment tests are *extreme condition* tests and are to this extent examples of the misuse of fertilizers. If this for the moment seems a poor *alibi* to the skeptical, I hope they will be patient.

This book is going to get very long if we analyze at length each individual set of figures but I think it is fairly obvious that these barley yields for Woburn soil illustrate much the same trends as the figures for wheat at Rothamsted.

Here are some similar barley figures from a long-term Rothamsted test. This time the grain yield is expressed in units of 1,000 pounds per acre.

	Average Yield in Consecutive Periods							
	First 5	*Second 5*	*Next 10*	*Next 10*	*Next 10*	*Next 10*	*Next 10*	*Next 10*
Treatment	*Years*							
Dung	2.3	2.8	3.0	2.9	2.7	2.6	2.5	2.0
NPK fertilizer	2.5	2.7	2.7	2.3	2.2	2.0	2.2	2.0
Nitrogen and potassium only	2.3	1.7	2.0	1.7	1.4	1.3	1.2	1.2
Nitrogen and phosphorus only	2.4	2.7	2.8	2.3	2.0	1.6	1.8	1.7
Phosphorus and potassium only	1.9	1.6	1.4	1.0	0.9	0.7	0.9	1.2

These figures, taken over seventy years, form a considerable piece of evidence. Note how the yield holds up at a higher level with the regularly applied dung, but do not jump to the conclusion that dung is better than chemicals because later it will be shown that this direct kind of comparison is meaningless in practical signficance. (At any rate, meaningless if it is presumed as a further deduction that chemicals must be abandoned in favor of dung.) In these tests the dung was applied at a rate that provided approximately as much active nitrogen, phosphorus and potas-

sium as in the complete NPK chemical fertilizer, and later we shall have to see just what that rate of application means before we can make sweeping deductions. The recorded fact is, of course, that for the continuous cropping involved in this long test, dung holds up the yield best, though in the end it drops down to about the same reduced level as that with complete fertilizer. Note that it takes quite a long time before the omission of potash in the treatment shows its diminishing effect. The effect of phosphoric acid deficiency shows itself more quickly, and so does nitrogen deficiency.

Now here are some figures from various tests at various places. The data this time is not necessarily long-term. I have quoted these collected results for they have been very neatly put together by a bureau that tried to bring research results to the knowledge of farmers, and they provide a neat demonstration of fertilizer effects.

Crop	*Yield with No Fertilizer*	*Yield with NPK Fertilizer*
Mangolds	27.8 tons	37.1 tons
Turnips	10.7 tons	18.4 tons
Potatoes	6.8 tons	9.9 tons
Barley grain	14 bushels	37 bushels
Oat grain	15 hundredweights	22 hundredweights

These yield figures are, of course, strictly comparative figures for the particular soils of the various tests. This is possibly stressing something that is obvious, but I am thinking of somebody pointing out, for example, that he can grow a better crop of potatoes per acre than the 9.9 tons above without any fertilizer, and, indeed, so he might on good potato-suited land with good inherent fertility. The point is that on this land—the land actually tested—the crop was only 6.8 tons without the fertilizer. Some growers are apt to form their opinions in this unprecise way, judging the results of some fertilizer treatment by comparisons of results on totally different types of soil. You can only form conclusions about the effects of a fertilizer by comparing its results on different soils if you do this on a very large scale—by compar-

ing the average yields at *a number of farms* where a fertilizer is used with those at a similar number of farms where it is not used. With so many unit results added up on either side of the comparison, there is then a good chance that the individual effects of other factors will on the whole cancel out.

We ought not to leave out grass as a crop although non-farmers so often think of grass as something that merely happens. Here are some figures showing the effect of fertilizers upon grass, from tests carried out at the Jealott's Hill experimental farm and research station as reported in a book by its one-time director, Sir Frederick Keeble.

	Spring Yield *pounds*	*Late Summer-Autumn Yield* *pounds*	*Total* *pounds*
With nitrogen	4,429	3,316	7,745
Without nitrogen	3,180	1,764	4,944

These weights are expressed as dry matter per acre. The weights of the fresh grass were totally: 18 tons with nitrogen against 10½ tons where no nitrogenous fertilizer was applied. Expressed (after analysis) in terms of protein content (that is to say in food value, though we must not here be dogmatic and imply that only the protein value counts in summing up grass, for there are other constituents of nutritional importance as well): with nitrogen, 246 pounds per acre—without nitrogen, 122 pounds per acre. Sir Frederick rightly pointed out that this showed that the value of the grass itself seemed greater where the fertilizer had been used; for the weight increase was about 75 per cent, and the protein content comparison shows an increase of 100 per cent. This test was for one season only, 1930. In the previous season, a season when grass growth was not helped so much by other variable factors concerned, weather, etc., the weight increase through the fertilizer application was not 75 per cent but 40 per cent. So we had better make the reservation of considering the increase-figure to be closer to the true figure, if we split the difference and call it 55-60 per cent.

If this test is left without further comment I lay the implied argument open to very strong criticism, criticism that was shrewdly voiced in Michael Graham's *Soil and Sense.* For this extra grass yield has not been obtained by the use of the nitrogen fertilizer alone. The greater growth has drawn more heavily upon the soil's reserve of phosphoric acid and potash, and this asset of the soil has been thereby reduced. Although admitting this point for the moment, I would rather not deal with it until it takes its more logical turn in a later part of the book dealing with the misuse of fertilizers.

Another interesting piece of evidence that we can take from Jealott's Hill is based upon the method of mass observation mentioned a few paragraphs before. Sir Frederick Keeble found quite a lot of people responded to his tests by saying: "Yes, very nice figures, but, of course, they're based upon small plots, not farms." The plot method of sample and analysis, even though conducted in the soil, has always met with resistance on these lines. So the Jealott's Hill staff tackled nine farms, and they applied greater amounts of nitrogen per acre than are used in general farming practice to the crops of these farms—totaling 2,500 acres—and they recorded the crops for 1928, 1929, and 1930. And these were the results. The column under England and Wales gives the average yield per acre for the country for the crop concerned, the other column the average yield per acre at the nine farms surveyed by Jealott's Hill.

Crop	*England and Wales*	*The Nine Farms*
Wheat	17.7 hundredweights	22.0 hundredweights
Barley	16.5 hundredweights	19.6 hundredweights
Oats	15.9 hundredweights	20.2 hundredweights
Sugar beet	8.4 tons	10.5 tons
Potatoes	6.9 tons	7.3 tons
Mangolds	19.1 tons	24.5 tons

Considered alone, this kind of test would be open to much criticism. To be safely independent of other variables, a mass observation of many more farms than nine would have to be

made. Also, it is not a fair control to take the average yield of all the other farms in the country, for this includes all the bad husbandry to bring down the average (as well as all the good to bring it up, of course)—and we should have to feel very convinced that the nine chosen farms were nine farms really representing a *fair* cross-section of the whole country's general agricultural standard of the time. Ninety farms would have been a better figure of course. Still, making skeptical discounts for these points, the figures do show—except for potatoes—a yield increase directly due to the use of fertilizers. Why the poor increase for potatoes? Sir Frederick's hint was that these nine farms were situated in a poor potato-growing area. Even so, the response should have been better, judging from the general run of these fertilizer test results. Another interpretation, for what it is worth, is this. The Jealott's Hill tests were mainly devoted to investigating the benefits of applying nitrogen in quickly acting chemical form. Nitrogen is not a dominant plant-food for the potato crop. We want enough leaf but we do not want excessive leaf; it is better for the plant to get on with the job of tuber development and carbohydrate formation. On these lines, one would not expect nitrogen applications that are relatively high (compared with phosphorus and potassium) to increase very successfully the final tuber yield.

While we are absorbing figures, we might as well make a thoroughly representative job of it. We ought to consider fruit crops. Now it is a far more difficult thing to plot-test fruit than to plot-test one-season farm crops. Fruit bushes and trees are permanent plants and their habits are affected by the whole of their histories. Obviously, the response of a tree to fertilizer treatment is a very complex business. Also, truly comparable orchards are much harder to set up than similar plots for growing wheat or barley or potatoes. Since the last war, Professor T. Wallace has been tackling this problem at the Long Ashton Research Station in Somerset. When it was recently said that the truth is nowhere more difficult to find than in agriculture, Professor Wallace's mission in horticulture was surely forgotten. For his evi-

dence must take longer to collect than ever Rothamsted needed for their work on farm crops, and his work needs greater space. Add to this the fact that the fruit crop is so often at the mercy of a far more dominating factor than nutrient supply—the late spring frosts—and you have some idea of the size of the problem with fruit. Nevertheless, much progress has been made in twenty years or so, and results show a trend not dissimilar to the Rothamsted trend.

Here are some Long Ashton figures for blackcurrants, average yields over five seasons. The yields here are expressed purely as comparable numbers, the crop from the no-manure plot being taken as 100 and all the other yields being expressed proportionately.

No manure of any kind	100
Farmyard manure	151
NPK fertilizer	148
Phosphorus and potassium, but no nitrogen	133
Nitrogen and potassium, but no phosphorus	130
Nitrogen and phosphorus, but no potassium	95

The most important aspect of this test is the clearly shown dominance of the potash effect. Once potash is not applied, the yield is catastrophically lower. A complete fertilizer seems to do almost as well as farmyard manure, but to be sure of this, we must consider a longer term than five years. However, at this stage we do not want to start long discussions on the relative values of farmyard manures and fertilizers. That must come later and in more detail. It is an odious comparison and one that inevitably misleads the unwary.

Here are some three-year test figures for Bramley Seedling apples, from another research station mainly concerned with fruit, East Malling, Kent. The sordid introduction of a cash column is essential to the proper interpretation of this test. As every fruit grower knows, quality and size and shape is a far more important cropping result than mere total weight of crop. A heavy yield of small apples (culls) is a poor yield compared with

a moderate crop of good grade fruit. In this test, the fruits were marketed, and the prices obtained were recorded.

	With Potash		*Without Potash*	
	bushels per acre	*cash value*	*bushels per acre*	*cash value*
Rootstock I	615	$616	502	$272
Rootstock II	387	$392	549	$296
Rootstock III	359	$292	264	$128

Comment upon these figures seems superfluous—they drive home their own story and need no text.

There is, of course, no limit to the sets of figures of this kind that we could quote. Scientific journals are littered with them. American research stations, with their much more liberal state aid, their larger staffs, and larger plantations, have been prolific with such tests. The results quoted in this chapter are only a sample of the kind of results that build up the case for fertilizers. Is it being dogmatic to conclude from this kind of evidence that fertilizers, properly used, increase crop yields over long periods as well as over short periods? Remember that many of these tests, particularly those at Rothamsted, are arduous tests. Few farmers would consider growing the same crop on the same piece of soil year after year. Yet, even though this strain of continuous, unrotated cropping tends to reduce the yield as the years proceed, the cropping level at the final steady minimum is higher where fertilizers have been used than where they have not. Can fertilizers *poison the soil* when steady cropping is achieved even after one hundred years of such extreme conditions?

As a further piece of evidence in regard to the long-term effects of fertilizers, a quotation from Sir John Russell's paper in the *Royal Agricultural Society Journal,* 1942, should be considered. "Discussion still continues on the ancient theme whether complete artificial fertilizers are foods or mere stimulants, sometimes even whether they are a blessing or a scourge. I can only say that the Broadbalk wheat plots receiving artificials only since 1843, and no farmyard manure since 1839, gave almost record yields

in 1939 and 1940." (However, it should not be supposed that Sir John Russell considers that natural manures can be abandoned, for in the same statement he also observes: "No agricultural expert who knew anything about his job, however, would ever advise farmers to use artificials only. . . .")

Nevertheless, sticking to logical argument, we cannot yet regard all the points indicated in this chapter as proved. The main point that does seem established is this: fertilizers applied to the soil increase crop-yields, and it therefore would seem to follow that the soil's own supply of available plant food is insufficient to maintain more than a low level of cropping. Or, put in another way, fertilizers are needed to increase our rate of cropping. As a deduction from this, it must be supposed that—*if* this rate of cropping is desired—the soil's rate of plant-food supply is inadequate for its attainment.

One last tailpiece of discussion. If the addition of so much fertilizer produces such-and-such an increase, why not double or treble the application and get a double or treble increase? It does not work out that way. The second addition produces a smaller response, the third smaller still—and so on until at a further stage the yield may even begin to fall. We are up against the law of diminishing returns.

This tendency, always measurable as a trend in the kind of experiment, where the plots compare increasing amounts of the same fertilizer, is a strong proof for the idea that the soil cannot by itself supply the necessary nutrients at the desired rate. For, if this inability on the part of the soil is a fact, then the soil is in a condition of deficiency. A falling off in response as fertilizer additions are increased is exactly what we should expect—for, as the externally applied plant foods get closer and closer to *the correction of the deficiency,* so their beneficial effects must decrease. And this is what does happen. Therefore, the idea of general soil deficiency in the three plant foods, nitrogen, phosphorus, and potassium, seems well confirmed. In the next chapter we must see what has actually been found out about the plant-food content of the soil itself.

Is it fair to talk about the soil as being deficient? Perhaps not. We use the word in a sense that is relative to our needs, not in any absolute sense. A larder in a small house may be very well stocked for the needs of a family of two, but, if the family is suddenly added to by half a dozen long-staying relatives, that same larder might be very understocked.

So much, then, for figures. I am well aware that the way is wide open for the criticism that figures can be made to prove anything, but surely this view of figures depends upon the assumption that unscrupulous tricksters are handling the figures before an audience somewhat lacking in common sense. That may partly be true of some departments in the structure of modern civilization, but it can hardly be believed of the scientific research department, which is in the powerful hands of men who are far more jealous of their professional reputation than of material considerations. Even if the early Rothamsted was closely linked (through Lawes) with a superphosphate producing factory, and, even if one of the more modern stations, whose work I have quoted, is closely associated with a large chemical combine, the work—the tests and the testing—have all been planned, controlled, and undertaken by scientists of the very highest caliber. I do not suggest even now that the deductions from these figures must be accepted as a kind of holy truth. We cannot afford to be dogmatic. There is the counterargument to be considered later. It must at least be insisted that these figures be given their proper place in the total argument and that this place is the expert witness box and not the dock.

CHAPTER IV

THE MINERAL CORPORATION

"Back of soil use and land planning . . . back of soil management by the individual farmer, lies a growing body of scientific knowledge regarding the true nature of the soil. The scientist working in this field would be the first to say that his knowledge is far from perfect—that there is more to be discovered than is yet known. But the practical handling of the soil must rest solidly on what scientific knowledge we do have. . . ." *United States Yearbook of Agriculture,* 1938.

THE SOIL'S own NPK supplies have so far largely been ignored. The qualitative effects of these nutrients have been investigated by water culture tests, thus entirely dismissing the soil. Still, the soil is a major raw material of agriculture, and it is a variable that cannot be dismissed or ruled out. Soils vary one from another as much as human beings, and perhaps rather more than we do in our increasingly robotized civilization.

Since it is known with some degree of accuracy, following the hundreds of field tests that have been recorded, what crop increases are likely to result from the addition of precise quantities of nitrogen, phosphorus, and potassium, is it not an easy matter for scientists to work out the NPK value of any soil from its cropping capacity, when no fertilizer additions are given? Indeed, to carry this logical deduction a little farther still, surely all the chemist need do is analyze the soil for nitrogen, phosphorus, and potassium, lime and organic matter, and from these figures work out the crop obtainable. Liebig and other early soil chemists thought and hoped along these lines too.

Unfortunately the task of science in revealing the mechanisms of the dynamic, natural world is never as simple as that. One door is opened, but inside there is only a short corridor leading

to another locked door. If each generation of research manages to unlock just one door, progress is rapid. The second locked door that faced the first soil chemists was the fact that their estimates of a soil's fertility did not fit in with actual cropping results. Fields with low NPK figures often grew better crops than fields with higher figures, even when other influences such as weather, husbandry, etc., were allowed for. They could, after collating the results of tests, predict with some reasonable accuracy the crop responses when nutrients were added to the soil, but they could not predict crop yields from their analysis figures for untreated soil. Yet to the scientist Nature could never really be as irrational as this.

The deadlock was obviously due to the fact that the NPK of the soil differed from the NPK in added fertilizers and manures. It was suggested that the soil's content of these plant foods could be divided into two kinds: an active kind easily available to plants, and an inactive kind not easily available to plants. Now the early chemical method of soil analysis was based upon treating the soil sample with strong acids to dissolve all the nitrogen, phosphorus, and potassium present, the extract then being tested. However, why, after all, should this method of extraction correspond in any way with the extracting capacity of a plant? Plants can do amazing things, push their new-born selves through baked clay surfaces and even through tiny cracks in stone, but hardly could they be credited with the extracting ferocity of powerful or corrosive acids. So a laboratory method was sought in which an extracting agent could closely imitate the gentler extracting power of plants. Note the assumptions in this argument: that there are two kinds of plant food and that there can be a solvent which can imitate in the laboratory the ways of a plant in the soil. How could such assumptions be justified? Only if, when such a solvent was found and tried, the results of the soil analysis could predict the actual cropping capacity of the soil. This type of argument is frequently the practice of research; to adopt a likely hypothesis, devise methods to measure its consequences, and then—*if* these measurements square with inde-

pendently observed facts—to work backwards and assume that the basic hypothesis must be true.

Many patient efforts were made to solve this problem, and the best of them was perhaps that devised by Dr. Dyer. He studied the acidities of a large number of plant-juices or saps and concluded that a 1 per cent aqueous solution of citric acid was about the nearest imitation to the average plant-juice. And in this Dyer method, the *available* amounts of phosphorus and potassium are assumed to be those that can be extracted by treating the soil sample with the 1 per cent solution. The non-available amounts are, of course, the total amounts as shown by the strong acid treatment less these available amounts. Note by the way that nitrogen has suddenly disappeared, for it is here that we have had to part the essential trio. Even today no satisfactory chemical method has been found that can reliably distinguish between active and non-active soil nitrogen.

This last statement implies that the Dyer method is nevertheless satisfactory for phosphorus and potassium. However, this is rather too definite an implication. The best one can say is that the method is moderately satisfactory. Its results do correspond to a fair extent with actual results in the field. In America a weak solution of nitric acid is preferred to the Dyer citric acid, this solvent giving results that correspond better with general cropping yields in the United States. This does not mean that there is a transatlantic division of chemical opinion. It means that the Dyer solution corresponds to our soil conditions as they affect a plant's extracting power, and, in the rather different American conditions, the dilute nitric acid solution fits the bill better there. For both solutions are quite arbitrary; and there is nothing that can be fundamentally or universally true about this choice of a laboratory mimic. Until we have a better idea, a rough working one must carry us along. What is wrong is for anybody to be dogmatic, to forget the arbitrary basis of the method and refuse to admit that its results can sometimes be *out of line*.

Naturally, the test of all this is the practical test. How often can the method be expected to predict with reasonable accuracy?

Is it any more reliable than tossing a penny or sticking a pin in a list of runners? At a meeting of experienced soil scientists not very long ago, it was suggested that the method could predict with very high accuracy cases of *good* and *bad* fertility, and be right in about four cases out of five (at the most) in less definite cases.

The importance of this aspect of soil science should not be underrated. Especially is it important in such emergency conditions as those of war, when large areas of neglected land are suddenly converted into arable land. In such a case a quick and reliable laboratory test may decide in advance whether a soil's fertility is worthy of the labor and seed, and so on, that are needed for the initiation of a crop, or it may determine by what means the fertility can be raised to make the effort successful. However, the soil scientist does not rely upon his laboratory alone in this work. He knows from experience what sort of Dyer figure for available phosphoric acid is likely to denote a rich fertility from this plant food's angle, and he knows also the kind of figure that denotes a low fertility. His problem is that these two limiting figures of *goodness* and *badness* are divided by a wide margin, and it is his unhappy lot that most soils he analyzes fall within this no-man's-land of uncertainty. It is almost the same with potash. (Of course, for different crops these figures will vary but he can allow for this.) His judgment in these cases must be built up by bringing in other factors. Local or regional conditions and his experience of them will be one very major factor, for he should know from past successes and failures whether the Dyer figures are generally optimistic or pessimistic for his area. This is made clearer, perhaps, by pointing out that plants' extracting capacities may well vary according to rainfall conditions, type of soil, and so on. In short, his special personal experience should help him to adjust the Dyer assumption so that it imitates Nature more accurately. Then again he may have to allow for the standard of soil care of the farmer, for this may well have some bearing upon the chance the crop has of actually obtaining the plant food that is theoretically available. Nothing will help him more

than an inspection of the soil itself and of the crop or wild vegetation actually growing upon it. He can tell then from the type of weeds or from the quality of the current crop whether there seems to be any specific deficiency or sufficiency in operation. A reliable report upon the past history of the field will help him too. Upon all these factors and not only upon the laboratory figures the soil adviser bases his opinion and recommendations.

It will be argued from this that the laboratory is of little use, that visual inspection and personal experience seem more important. In this double statement, the latter sentence is not only true but increasingly true. However, it would be unfair to assume that this knocks out the laboratory. Much of the soil scientist's experience has been gained by integrating field observations with analysis data. When visual examination tells him that potash deficiency exists, it is because in similar cases he has been able to check such evidence with laboratory facts. Perhaps the soil has scored a partial victory over the usual certainty of balance and burette, but who can reasonably expect any method (or any small number of methods) to assess the many variations in so complex an equilibrium as that of the soil and the conditions that affect it? That the soil scientist is right four times out of five or even three times out of four is good progress. If it is not as reassuring as many would wish, it can only be said that it is the hard truth. Are doctors' diagnoses of difficult cases any better? Yet medical science is many centuries older than agricultural science.

I have stressed these difficulties of the accurate scientific assessment of soil fertility because it seems important that the layman—the client of science—should understand them. Far too often is the man with letters after his name and a laboratory in the background expected to produce a foolproof solution. When, after what might seem a mystic procedure, his advice turns out to be rather wide of the truth of later events, the recipient of advice tends to damn all scientists for a durable portion of eternity. Yet the problem may well have been beyond the present capacity of science to solve. Then why didn't the technical man say so? Well,

he may not have been able to decide the full scope of the problem at the time. What he thought to be sound judgment may have been torn into shreds by the influence of some obscure factor. Similarly, when the technical man is right, the farmer often concludes that science is in future the certain cast-iron remedy for all his troubles; yet here the problem may have been quite simple and needing very little knowledge. Right or wrong, cursed or praised, the soil investigator must persevere, and he can be helped in this uphill journey only if the ordinary man understands his difficulties and perseveres too. In the cooperation of research and practical farming there are two obstacles—the overdogmatic scientist and the impatient layman. And there have tended to be too many of both species.

However, to return to the soil. Accept for the time being—as we must—that we can be about 70 per cent right in estimating the amounts of available phosphorus and potassium in soils. What is the difference between the available amount and the non-available, locked-up amount? Now this is a point at which there can be considerable argument between the fertilizer school of thought and the antifertilizer school. However, we had better stick to the orthodox view for the moment. I am fully aware that this chemical view of soil fertility is by no means everywhere acceptable, but I do not see how the matter can be usefully debated until the chemical thesis is first explained.

It can be said at once that the non-available quantities are vastly greater than the available quantities. Here are some figures from Cornell University in the United States. In the soil studied, the figures for total phosphoric acid in the top 4 feet of soil showed a content that could, *if* it were all available, support normal rotation cropping for 367 years. For potash, the calculated period was 1,435 years. Sir Daniel Hall stated, "Roughly speaking, an average soil contains enough plant food for a hundred full crops." There is no need to worry about the differences in scale of these two assessments. In potential phosphorus and potassium, any soil is rich—and estimates of the degree of richness matter about as little as the difference between a fortune of

$50,000 or $500,000 matters to the ordinary poor man. Both amounts of bank balance spell real wealth. In a reasonably rational world it wouldn't matter to the rich man himself whether there were the five figures or the six. It would worry the rich man very much indeed if, despite having this kind of credit at the bank, he were allowed to draw only 40 dollars a week, especially if his running expenses were 80 or 120 dollars. And that would seem to be the problem of the soil. There is a regular conversion of non-available plant food into available forms, but its rate is often less than the rate of consumption imposed by the human demand for crops. The use of lime is often regarded as a trick that can be played upon the soil to increase the rate of availability, but in correct perspective the function of lime is not so much one of acceleration as of prevention of diminution. Soil acidity reduces the conversion of insoluble nutrients into soluble forms, and lime additions only correct this.

The action of lime is chemical rather than biochemical in regard to phosphorus and potassium. The mineral matter of the soil is very largely made up of complex insoluble silicates. Various *basic* elements are combined in these complex *salts* with acidic parts, e.g., the silicate part. However, the bases can be turned out of these salts by other bases. It is a fairly complex story but a simplified version will serve the argument's purpose. Thus, lime will displace some of the locked-up potash in these complex silicates, and this potash will then be in an active, available form in the soil. Similarly, it is considered that the addition of sodium compounds (such as common salt, sodium chloride) will make more potash available, for some of the sodium will displace some of the locked-up potassium. However, it should be remarked that not every authority in soil science agrees about this. With phosphoric acid supply, the favorable effect of lime depends upon the fact that it keeps the phosphate part of these mineral compounds attached to calcium, and calcium phosphate is more reactive than iron and aluminum phosphates, which are formed in absence of lime and which are insoluble and non-reactive. Lime, in short, displaces the iron and aluminum which under acid

conditions would have combined with the phosphate *radical* and thus locked up the phosphate.

It should not be thought that these chemical functions are the only reasons for using lime once every three years or whenever acidity measurements show lime to be needed. There are other reasons too, notably to counteract the adverse effect of acid conditions upon micro-bacterial activity.

However, in this discussion, a general picture of phosphorus and potassium dispositions in the soil has emerged. As phosphates, phosphorus may be combined with calcium, and this calcium phosphate, though insoluble, will be slowly converted by the soil into more reactive forms. On the other hand, some of the phosphorus may be in the form of the phosphates of iron and aluminum, which are very insoluble, and this proportion of the total phosphorus will be non-available. Similarly, potash will be in complex silicate forms that are locked-up, but the severity of chemical imprisonment will vary; and thus the rate of conversion into available forms of potash will vary.

This can be expressed in simple equation form as follows:

Total phosphorus in soil = (non-reactive phosphorus) + (very slowly reactive phosphorus) + (active phosphorus in soil solution).

This expresses a static situation, whereas in truth the situation is one of dynamic equilibrium.

The non-reactive locked-up nutrients probably remain so. But the very slowly reactive kinds are steadily providing a small part of their substance as additions of active phosphorus and potassium to the soil solution. On the other hand, some of the active phosphorus and potassium in the soil solution is continually being reverted by chemical actions in the soil to these less desirable forms, either to only slowly reactive or to non-reactive forms. The *total* amount of effective phosphorus and potassium from the soil's store is *the difference between production and reversion.*

The addition of lime, as we have seen, will help by turning some of the non-reactive phosphorus and potassium into very

slowly reactive forms, and in the case of potassium into directly active potassium to some extent. Cropping, however, will naturally cause steady removal of active phosphorus and potassium, and adverse disturbance of the equilibrium. It may not be so adverse as this simple account suggests, however, for we may have the situation that the soil solution before crop removal is holding all the active phosphorus and potassium which the equilibrium permits. Further supplies of active phosphorus and potassium from the soil's store would not be forthcoming then; the active material would be reverted as soon as new active material would arrive, thus keeping the total situation in an *as you were* state. The complete removal of any active phosphorus and potassium from this equilibrium would then enable a new flow of active phosphorus and potassium to occur from the inactive side. This kind of compensation is limited. The rate of cropping removal may be high; the maximum rate of compensation bears no relation to this, it is a rate whose maximum is settled by the soil conditions only.

When we use fertilizers, we are using forms of phosphorus and potassium which are usually very reactive, similar forms to those that exist in the soil solution; or, at the least, forms which are slowly reactive. Fertilizer supplies of phosphorus and potassium, therefore, are *initially* powerful additions to the active end of the story. We must say *initially* because, unless crops utilize the supplies quickly, soil reactions may take place, and slowly revert some of the fertilizer phosphorus and potassium to less active forms, eventually even to non-reactive forms. In short, the added supplies become absorbed into the soil systems of equilibria. Fertilizer application, at the lowest estimate, is a temporary disturbance of soil equilibria in favor of what seem to be the plant's immediate chemical needs.

It should now be clear that the phosphorus and potassium fertilizers cannot be regarded as chemically similar to the total phosphorus and potassium in the soil, but only to the more active fractions of the phosphorus and potassium in the soil.

It can be counterargued that regular disturbances of Nature's

equilibria will reduce Nature's capacity to play her part. Thus, the regular use of indigestion powders may remedy attacks of indigestion, but this artificial procedure is said to reduce the human system's natural capacity to remedy indigestion itself; and consequently attacks become more frequent. Similarly, if the soil solution is frequently filled to a maximum with soluble phosphorus and potassium by fertilizer addition, the steady natural flow from the soil's store is depressed because an already saturated soil solution will accept no further additions from any source. However, this view hardly fits the case, for there is also this constant removal of the nutrients by the plant—a removal right outside the soil equilibria, into the chemical factory of the plant. And the increased cropping induced by fertilizers involves a greater rate of removal. All the long-term evidence of the kind quoted in the last chapter supports the view that fertility in the chemical nutrient sense is increased by the regular use of fertilizers, and it does not seem possible in face of these facts to consider that the soil's own contribution is at the same time diminished, for, if it were diminished, the crop responses in long-term tests would themselves diminish much more sharply season by season. When the Dyer test is applied to soils that have been previously treated with fertilizers, increases in the contents of active phosphorus and potassium can be measured.

The soil's capacity to release active phosphorus and potassium from its large locked-up or partly-locked-up stores is not enough; not enough *for our demands upon the soil for the amount of food we need to maintain the dominance of the human species.* That it may be enough for Nature's own purposes is quite another story—Nature and Man are not always aiming at the same objectives. The addition to the soil of external supplies of active phosphorus and potassium is an essential procedure in the arrangements of modern civilization, arrangements which include the maintenance of a large industrial population which is not food producing. The value of the soil's capacity for cropping without fertilizers was measured over centuries when farming depended upon dung and lime alone. To quote the late Sir

Daniel Hall again: ". . . the skilful use of farmyard manure and legumes brought the level of productiveness of our English soil up to twenty bushels of wheat per acre—the use of artificial fertilizers then raised it to thirty bushels."

It must be pointed out that the preceding ideas about what the soil can and cannot do for us are a mixture of fact and hypothesis, and, as such, the argument presented must not be regarded entirely as proof positive for fertilizers. The facts have been these:

1. Increases shown by additions of fertilizers over crops produced from soil resources alone.

2. Chemical distinction by arbitrary tests between active and less active fractions of the total phosphorus and potassium nutrients in the soil.

3. Independent chemical knowledge that certain forms in which phosphorus and potassium are found in the soil, e.g., iron and aluminum phosphates and complex silicates, are very insoluble.

4. Crop increases shown by lime treatment of acidic soils, and independent chemical knowledge that lime will break up these insoluble compounds.

From this theory it follows that external additions of active phosphorus and potassium fertilizers are essential unless a low rate of cropping is sufficient for the human purpose.

CHAPTER V

THE NITROGEN SPIRAL

"The fixation of nitrogen is vital to the progress of civilized humanity, and unless we can class it among the certainties to come, the great Caucasian race will cease to be foremost in the world, and will be squeezed out of existence by the races to whom wheaten bread is not the staff of life." SIR WILLIAM CROOKES, 1898.

"The proportion of the world's output of nitrogenous fertilizers made by nitrogen-fixation processes is now about 75 per cent, expressed in terms of nitrogen." From *A Century of Fertilizer Progress,* by E. H. TRIPP and S. W. CHEVELEY, 1939.

SCIENTIFIC TEXTBOOKS of a general type frequently describe the *nitrogen cycle* with: nitrogen passing into the earth from the air through some agencies, and nitrogen returning to the air through other agencies. Generally, these textbooks pass on after a few comments to consider the chemical properties of nitrogen and its compounds, and thus the impression is easily left that nitrogen is circulating very nicely between earth and air like the hot water in a closed central heating system between boiler and radiators. There certainly is a cycle and it is vital to the world. From the sense of human requirements, the cycle is no balancing cycle at all. Let no one suppose that a fixed and sufficient amount of nitrogen is comfortably going backwards and forwards out of and into the soil. Until in this century we discovered methods by which we could *fix* atmospheric nitrogen in factories, the maintenance of nitrogen supply in the soil was considered to be one of the world's most urgent problems, and scientists foretold a future of dwindling food production.

The air contains 80 per cent nitrogen. It has been calculated that there are 150,000 tons of gaseous nitrogen over every acre

of ground. There is, therefore, no shortage of the raw element. Inert elements combine with other elements only under powerful persuasion. And nitrogen by itself, contrary to Liebig's original idea that plants absorbed nitrogen through their foliage from the air, is useless as a plant-food. Plants feed mainly upon nitrates, though they can feed upon ammonia forms and in some cases upon more complex compounds. Leaving out the question of additions of nitrogen to the soil by fertilizers, the nitrogen problem involves these factors: (1) minimizing losses in the soil's working capital of nitrogen; (2) keeping a good proportion of this capital in an active, mobile form that can be used when needed, and (3) pulling in some of the air's enormous but inert supply to reinforce the dwindling capital. If these things can be done naturally without reduction in our cropping, clearly there is no case for the additional use of nitrogenous fertilizers. It is not enough to say that field tests and experience have shown that additions of nitrogen lead to crop increases; even though this frequently recorded fact would seem to prove that the nitrogen cycle is a cycle of quantitative loss. We must, if we can, find out *why* and *where* the cycle leaks.

In considering the ways in which nitrogen leaves the soil or gets into it, it will not be possible to provide full details of how these mainly qualitative matters have been found out. To do this would turn a chapter into a book. The research work of the chemists and the biologists must be taken on trust, and skeptical readers will have to refer to other books in which the evidence has been set out.

First, are there any ways in which the soil can obtain nitrogen from the air? There are two: an electrical and a biological. A discharge of lightning will induce tiny amounts of air nitrogen to combine with oxygen, and the soluble compound produced is absorbed by rain and this then falls upon the soil. This is easy enough to prove and measure by collecting rainfall and analyzing it. Over 28 years of such measuring at Rothamsted, the figure for this factor of gain was 3.97 pounds of nitrogen per acre per year. At Ottawa, in a ten-year determination, it was 6.58

pounds per acre. For our climate, then, and it clearly depends upon how many storms we enjoy, we can reckon upon an annual gain of about 4 pounds.

The biological fixation of air nitrogen is performed by bacteria. Bacteria, though they are so small that many millions inhabit a spoonful of soil, are chemically much cleverer than we are. They can turn one substance into another in conversions that we either cannot perform in laboratories at all or can do so only with a most complex arrangement under special conditions. Yeast, for example, will ferment juices, turn the sugars into alcohol; and other kinds will then proceed to turn the alcohol into acetic acid. This is a bacterial action. Cheese making is a bacterial action. In recent years chemists have begun to use bacteria on an industrial scale to make certain substances with their help because in some instances this is easier than the more obvious chemical processes. We cannot wander down this intriguing by-road, but those who would like to make the trip should read Dr. Nicol's *Microbes by the Million,* a masterpiece of simplified science. We must content ourselves with the short view that bacteria are ridiculously small organisms that thrive in teeming colonies and feed upon certain substances, digest them, and thus turn them into other substances, which are then expelled from the bacterial system during life or at death.

Two different types of bacteria have the capacity to feed upon the air's nitrogen and turn it into complex, combined forms of nitrogen. One type is independent, the other is associated only with certain kinds of plants as a parasite. The independent kind of these bacteria is called *azotobacter.* They are present generally in soils, and this would seem to make them more important than the other kind that depends upon the growth of specific crops. Importance is a matter of quantity, after all. Men who could grind holes in metal with their teeth would undoubtedly be useful to industry especially at a time of machine tool shortage, but their value to the total industrial problem would depend upon how many there were of them and whether they could work hard without special conditions. The azotobacter

do not seem as numerous as other kinds of bacteria and they work only under certain conditions. They are inactive when it is cold and they go on strike at quite a moderate degree of acidity, a degree that would be comparatively unharmful to many crops. The amount of nitrogen these bacteria bring to the soil is therefore relatively small. This, at any rate, is the conclusion of the majority of microbiologists. To estimate any reliable figure for their average contribution is difficult in view of the variation with conditions, but, as we are arguing out a case for nitrogen addition by other means, we had better err on the generous side toward the azotobacter. Twenty-five pounds of nitrogen per year per acre under favorable conditions is probably a better figure than the actual contribution as a general average.

The other kind that lead lodgers' lives can be rather more profitable. They inhabit the nodules that are found on the roots of peas or beans. Clover and vetches also act as landladies to these bacteria. The plants that attract these paying guests are known as legumes. Long before this bacterial knowledge had been deduced, it was known that these leguminous crops were soil enriching rather than soil impoverishing, and considerable use was made of them in the traditional practices of crop rotation. Although these root-inhabiting bacteria work hard to *fix* nitrogen, it must be remembered that their host-plants want nitrogen as food, and they naturally consume this bacterial production, which is in the right place at the right time. This means that the soil is not much enriched *unless* the crop, or part of the crop, returns to the soil. The plant residues will be rich in nitrogen that has been won from the air, and every gardener who has ever planted winter greens where he dug in the remains of his early summer peas will have observed the favorable effects upon the winter crop. In those parts of the leguminous crop that are permanently removed from the soil that proportion of the nitrogen gain is no gain at all. That is why clover is so important a crop or part-crop in good husbandry. It is an excellent hay crop, its nitrogen has been acquired for nothing, and the nitrogen that the cattle does not retain for their own constructional needs is

returned to the soil as a manure constituent. Like the azotobacter, these bacteria dislike acid conditions and that is why leguminous crops should always be grown on well-limed ground. How much nitrogen can we expect from this bacterial effort? Again, assessment is not easy. So much depends upon conditions, upon how much of the fixed nitrogen is crop-removed or soil-gained. Taking higher rather than lower estimates, a figure of about 80 to 100 pounds of nitrogen can be reached per acre per year. This gain depends upon the deliberate growing of a leguminous crop, and in rotation cropping this would not be more than once in 4 years or so. So we must at least divide by four, and our annual figure becomes 25 pounds at a favorable estimate. Where farming practice ignores the legumes, the figure is, of course, nil.

This concludes our known natural methods of increasing the nitrogen working capital. How about the actual capital in the soil? This is almost wholly to be found in the organic matter of the soil, in the accumulation of plant and animal residues. The nitrogen in these substances is combined largely in complex, insoluble kinds—higher proteins, lower proteins, amino-acids, and so on. These names matter little anyway. The soil contains billions of various kinds of bacteria whose lifework seems to be the breaking down of this complex inactive nitrogen into simpler, soluble forms. Until this bacterial work has been performed, plants cannot feed upon the nitrogen. The simplification process eventually arrives at a first simple stage of ammonia-form nitrogen, and this is followed by further stages in which the ammonia is oxidized to the nitrate form. It is often sweepingly asserted that plants have to wait for this nitrate form to turn up before they can feed at all, but more detailed study of the research work on this subject reveals that plants can to some extent feed upon the ammonia form and even upon more complex, organic forms. However, the sweeping statement is nearly true—nitrate nitrogen is the predominant form.

The important question is, how much of the inactive stock of nitrogen is turned into this active nitrate nitrogen per year? These bacteria also object to acidity; so liming helps them to

work harder. They like a reasonable air supply for they must have oxygen. If the air supply is poor, as in waterlogged soil, their activities cease and they are replaced by bacteria that can take oxygen out of the organic matter itself, bacteria that are associated with putrefaction. These less pleasant bacteria will even take oxygen out of nitrates and thus turn the combined nitrogen into free nitrogen, which then leaves the soil and returns to the air. So well-aerated soil helps the *right* kind of bacteria. These bacteria are also unhappy in cold weather; they intensify their efforts in the summer, but they do very little in the winter. They need some moisture so that a dry spell may reduce their working capacity. So, with all this sensitive dependence upon conditions, it is impossible to determine at any given time the rate of change from inactive to active nitrogen. It might be thought that chemical analysis of the nitrate nitrogen in the soil solution would provide quite accurate data from which to deduce this information. The nitrate content is erratic; it varies with conditions and not just with one set of conditions but with several. Measurements have only a kind of day-to-day significance and the figure for Monday might be very different from the figure for Saturday.

There is one aspect of this bacterial activity that should be stressed. All work requires energy for its performance; there is no widow's cruse for this. When the plant's leaves take carbon out of carbon dioxide, they draw the required energy from the sun, for the process stops if light is withdrawn. The bacteria live in the darkness of the earth. They derive their energy from the carbonaceous matter in the soil's organic material just as much of our own energy is derived from the breaking-down of carbohydrates such as sugars and starches. Therefore the bacteria depend upon a supply of organic matter not only for the nitrogen supply, but also to get the energy with which they convert the more complex nitrogen compounds into simpler ones. The more organic matter they have the more they thrive and work.

There is, however, no gain in total nitrogen in this section of the nitrogen story. However efficient the bacteria, they cannot,

like the azotobacter, make more nitrogen than is contained in the organic matter of the soil or added to the soil. If we grow a crop and remove so much nitrogen, that amount is taken from the soil; and only a portion of it is put back, that portion in the part of the crop returned to the soil as manure or crop residues. The cattle keep some nitrogen in their own system; and also some of the nitrogen they reject is lost in the making of farmyard manure. Human feeders throw away all the nitrogen that they do not retain. Bacteria cannot balance this deficit, they can only do their best according to the quota they are given.

To sum up, we have these changes taking place:

1. Gain from air by storm rain, about 4 pounds per acre per year.

2. Gain from air by azotobacter, variable but up to 25 pounds per acre per year.

3. Possible gain from air if a leguminous crop is grown and if the crop is wholly or mainly returned to the soil--for one such crop each four years in rotation, up to 25 pounds per acre per year.

4. A losing equilibrium between complex, inactive nitrogen and simple, active nitrogen, in which plants turn the simple into the complex, and in which bacteria turn the complex back into the simple. However, loss is involved since a considerable proportion of the plant-taken nitrogen never returns to the soil.

The balance sheet is not yet complete. There is at least one further factor of loss to be considered, and this time it is a nonbiological factor. The active forms of nitrogen, ammonia or nitrate forms, are very soluble in water. So are potash and phosphoric acid in their active forms, but they have the complex silicates and the calcium, iron, and aluminum compounds respectively to *fix* them in slightly soluble or insoluble forms and so hold them in the soil. Nitrogen seems to have fewer friends. As if to counter-balance all this helpful bacterial activity, very poor *chemical* arrangements exist for the soil retention of soluble nitrogen. To some extent the ammonia form can be held on to as

a constituent of complex silicates, but the nitrate form has not even this asset. So, unless the active nitrogen forms are taken up by plants, they are likely to be washed well down into the soil and then right out of the soil by rainfall. The drainage ditches of fields are carriers not merely of surplus water but of lost nitrogen.

We cannot measure this loss in any general way. Like the other changes in the nitrogen equilibrium, it depends upon many factors. Consider a wheat field harvested in the late summer. At such a time the sun has brought the earth to its maximum temperature so that in all probability the nitrifying bacteria are making their maximum effort. The field immediately after harvesting the crop is more or less empty of nitrogen-using plants. Only the stubble remains. If heavy rainfall suddenly descends, the nitrogen loss will be great. This loss could be very much reduced if what is called a cover crop were growing after the wheat had been cut. This crop would consume much of the active nitrogen then available, and later plowing in or cattle-feeding would return it, or most of it, to the soil as complex nitrogen. Thus, the amount of loss by leaching is dependent upon husbandry policy as well as upon weather. Clearly also, the loss must depend upon the amount of active nitrogen in the soil. A soil that is turning its total nitrogen into active nitrogen very slowly will offer less nitrates to the rain for removal. Such a soil will not be good soil for general cropping either.

A forty-nine-year experiment at Rothamsted attempted to measure the total nitrogen loss from soil under continuous wheat crops, and its figures give some idea of kind of loss we are up against. We cannot use the figures as a general argument for it would be illogical to build up a universal deduction from a particular case. Also, what can happen under continuous cropping may be much worse than what happens under rotation cropping.

The measurements and calculations in these tests are all so simple in kind that they can hardly be distrusted or devalued. They are simply matters of sampling and analysis and multiplica-

tion by the total weights involved. The nitrogen content of the top 9 inches of soil has been considered. Figures given are those for an acre.

On one plot the only manuring was with farmyard manure each year. This gave to the soil 201 pounds of nitrogen per year. To this supply must be added 7 pounds to cover the rainfall contribution and the nitrogen in the seed sown. Therefore, each year, the soil totally received 208 pounds of nitrogen. However, each wheat crop removed 50 pounds of nitrogen per year. So the soil should have gained 158 pounds annually. In 1865 the total nitrogen content in the soil per acre was 4,850 pounds; and, by 1914, it was 5,590 pounds. A gain in 49 years of 740 pounds, which works out at only just about 15 pounds per year. The theoretical annual gain of 158 pounds is reduced in fact to a mere 15. There was, therefore, *an average loss of 143 pounds of nitrogen per year.*

On another plot, similarly treated except that no manuring of any kind was ever performed, the only annual nitrogen addition was the 7 pounds from storm rain and the seeds. The crops, smaller because of the deliberately poor conditions, removed an average of 17 pounds of nitrogen per year, so here we have no calculated gain in total nitrogen but a loss of 10 pounds per year. In the 49 years of the test, the nitrogen content of the soil dropped from 2,960 pounds to 2,570, a fall of 390 pounds—that is, just about 8 pounds per annum. So here we arrive at an actual gain in nitrogen. Ten pounds per year have been lost from balance sheet calculations, but in actual fact only 8 pounds per year have really gone. As we have been ignoring the incalculable intake by azotobacter, we can—if we want to take this small gain figure of 2 pounds per year seriously—assume that they had something to do with it. If we bring in the azotobacter, it really means increasing our figures of change by some unknown amount, for we must then add their unknown contribution to the credit side of the budget.

However, we have this important fact revealed. With farmyard

manure applications there is an unaccountable loss of 143 pounds (plus the unknown azotobacter quota); where there was no manure of any kind, the position was little changed. We seem entitled to deduce that in the *starved* case, the wheat took all the active nitrogen there was, and so there was never very much left to be lost to other nitrogen-removing factors.

Next came two plots where the soil was treated with NPK fertilizers, each year's supply containing 86 pounds of nitrogen. We must take two plots here, for variations in yields were experienced. Adding the constant 7 pounds as before, the additions were 93 pounds per year. On one plot the crops removed an average of 46 pounds per year. So here there was an annual excess of 47 pounds. For this plot, however, the 1865 and 1914 nitrogen contents were 3,390 pounds and 3,210, a loss of 180 pounds, or about 4 pounds per year. A factual loss of 4 pounds against a balance sheet indicated gain of 47 pounds—a loss, therefore, of 51 pounds per year. On the other plot, lower crops removed only 44 pounds per year, thus leaving 49 pounds to the soil. Over the 49 years, however, the total nitrogen content fell by 80 pounds, that is by about 2 pounds per year. So again the real loss was 51 pounds. The agreement of these two figures despite the differences in yields is a nice indication that we are dealing with a sound method of measurement.

It might be argued that the biggest loss, that of the manured plot, is due to the fact that here much larger doses of nitrogen were given, and therefore there was more excess to be leached away. Actually, those who planned the experiments chose to give this relatively greater amount of nitrogen in the farmyard manure series because they were adding nitrogen in a complex, inactive form; whereas, in the fertilizer plots, the nitrogen was all or mainly in soluble, active form. At any rate, the crops yielded are comparable with only a shade of advantage to the natural manure.

Here are indications of very considerable loss. There is no leguminous crop to help us, but we must reckon we had the help

of the azotobacter, the storm rainfall, and the nitrogen in the seeds. Except where there was so little nitrogen about that the wheat took it all, the net annual losses were severe, at least 143 pounds per acre or 51 pounds per acre respectively, about two-thirds of the manure or fertilizer applications.

Other measurements of the actual net changes in total soil nitrogen have been made. It is general experience that where the ground is under unharvested vegetation continuously, such as in natural conditions of wild grass, weeds, etc., there is no loss, but a gain. This gain is not only in nitrogen but also in organic matter. This is not surprising. Nitrogen is not removed from the soil in harvesting, and the gains from the air via storm or microbes are being added. Each plant when it dies gives its nitrogenous remainders slowly back into the soil from which it originally took the nitrogen. The loss by drainage is unlikely to be great since there are always growing plants to claim priority upon any soluble nitrogen available at any time. The gain in organic matter also is understandable. The leaves are always *fixing* carbon, and so each generation of plant life adds to the soil a quota of decomposable matter. However, as soon as we begin to cultivate land by plowing it up and growing spaced crops to take advantage of this accumulated fertility, we introduce nitrogen loss. Here are some American figures that show this effect for prairie land.

When the prairie land was plowed, its nitrogen content was 6,940 pounds per acre. After 22 years' cropping, this figure had dropped to 4,750 pounds. Loss–2,190 pounds. However, over these 22 years, the nitrogen removed by the crops was known to be only 700 pounds approximately. So there was an unaccounted loss of 1,490 pounds in 22 years, or 68 pounds per year per acre. No considerations are made here of additions by fertilizer or manure since the cropping was carried out without these, presumably as an effort to grow food upon the land's store of fertility. Add to this net loss of 68 pounds per year the nitrogen gains there should have been from storm and azotobacter activity, and the real loss must be more than even this apparent amount.

Is this loss due entirely to drainage removal? Rothamsted has another long-term experiment to provide evidence on this point. A small plot was turned into a scientific instrument by cementing its drainage ditches, thus ensuring that all its drainage waters could be collected, measured in amount, sampled and analyzed for nitrogen content. At the same time, the ground enclosed by these ditches was kept free of any plant-growth. Expressing the nitrogen content on a scale of pounds per acre—though the plot was actually only one-thousandth of an acre in size—the nitrogen content fell from 1870 to 1917 by 1,148 pounds. The sampling and measuring of the drainage waters showed that, in the soluble nitrate form, 1,223 pounds of nitrogen had been lost from the soil by this cause. Allowance was made for the amount of nitrogen brought down by rain. We cannot expect an experiment of this kind to add up to a decimal point balance, but it certainly seems to establish the fact that for uncultivated, bare land the nitrogen loss is almost entirely due to nitrate nitrogen being washed from the soil.

The rates of loss showed that the rate was higher, about 40 pounds per year, in the early stages, falling to a steady annual figure of about 25 pounds per year in the end. This seems easily explained. Before the ground was bared, it had been arable land, and it therefore started off fairly full of bacterial colonies. As the years of bareness went on, these bacteria must have diminished in number and activity, for there were no plant-residues to supply them with organic matter as energy-food. Therefore the rate of production of active nitrogen declined, resulting in less soluble nitrogen to be washed away.

This chapter is becoming overloaded with measurements of nitrogen changes, but the issue is perhaps the most vital of all issues we have to consider. Here are some more figures from Rothamsted. Two plots were allowed to run wild with uncontrolled vegetation for 20 years. One plot had a high lime content, the other a poor lime content. The limed plot showed an average nitrogen gain of 92 pounds per acre per year; the other showed 60 pounds. This clearly proves that bacteria will do their job

better in conditions of low acidity. We must, however, assume that wild leguminous crops played some part in these nitrogen gains, for otherwise the figures imply rather higher azotobacter and storm rainfall gains than other measurements generally indicate. This data confirms the ancient practice of fertility restoration by leaving cropped ground to fallow; indeed, in the middle ages it was law that ground should be fallowed 1 year in every 3. Today the practice is to add sulfate of ammonia, and in the sense that this in wartime may be directed by a War Agricultural Committee this too can be said to be law. The implication in these two paralleled statements will make the antifertilizer school groan and fertilizer manufacturers smile happily. However, after all, sulfate of ammonia was not available in the middle ages. There was, too, a smaller population to be fed and a much greater proportion of this population working upon the land, and keeping one third of the agricultural land idle was not necessarily uneconomic. However, I have been tempted here to be provocative. To restore the balance, it should be pointed out that War Agricultural Committees today are also ordering reseedings to grass where arable land has been successively and heavily cropped, which is an equally traditional method of nitrogen restoration.

Coming back to this major problem of nitrogen loss, we seem to have these points fairly well established:

1. Totally there is a serious loss when soil is cultivated. This loss seems to be due to considerable nitrate removal by leaching, and to steady loss by crop removal. Bacterial gains from the air, etc., do not seem able to balance this. The loss can be minimized by crop rotation if the rotation includes a leguminous crop, and if this crop is wholly or mainly returned to the soil; also, by the growing of cover crops whenever there is a period of freedom between the main crops, these cover crops to be returned to the soil either directly or via cattle.

2. If ground is left to run wild or is under permanent vegetation that is not consumed off the farm, there is a gain in both nitrogen and organic matter.

3. There is a most wasteful loss by drainage whenever soil is left bare.

These points concern only total nitrogen, and crops depend upon the amount of active, soluble nitrogen.

4. The effective nitrogen content of a soil depends upon the activity of bacteria, and this in turn depends upon a good supply of organic matter and upon liming to counteract acidity.

When we look back at the traditional methods of farming, the four-year rotation policy, the diversified farming policy, the alternation of periods of arable cropping with periods of grass or ley farming, we cannot miss the close relationship of all these practices with the factors of nitrogen change. Yet science has been able to present this account of the matter only over the last 100 years and mainly in the last 50, and these farming *laws* were established long before. Some of them, indeed, were law without inverted commas. The law being wisely aimed at the maintenance of soil fertility as a national property no matter who happened privately to be holding the deeds or paying the rent. When factual observation over several centuries is confirmed by scientific research in a later century, we surely stand upon firm ground.

All this, however, is about, or mainly about, nitrogen losses and gains without additions by fertilizers. The nitrogenous fertilizer seems to win its place in two ways. First, it can help to balance the total net loss in arable farming. Second, it can help to increase the amount of active nitrogen where the conversion of complex nitrogen seems too small and slow for our needs. These are deductions that may reasonably be drawn from the various details of the nitrogen story. And they seem to be confirmed by the results, particularly the long-term results, of additions of chemical nitrogen. For somehow we must try to balance this net loss of nitrogen from the soil, and it seems difficult to throw the whole problem upon bacteria and years of leys. The balancing of nitrogen losses in arable cropping would mean long terms of ley farming, for the rate of loss is high and the rate of restoration under permanent vegetation is not so high. Also the

rate of restoration is reduced by the amount of nitrogen taken and not returned by grazing cattle. Fertilizers can shift the point of balance, can lengthen the period of arable possibilities and shorten the restoring period of non-arable farming. There are some who go even farther, who contend that fertilizers can shift the point of balance so far toward arable farming that these other methods of nitrogen restoration can be given up. When we go so far as this in claiming fertilizer benefits, we must not forget that other factor in soil fertility associated with alternate leys, the building-up of organic matter in the soil. Can the organic matter content be sufficiently maintained if we settle the nitrogen losses by heavily applying chemical nitrogen and adopt what some critics describe as *continuous monoculture?* Is continuous arable cropping safe from the humus balance-sheet angle? We cannot ignore the fact that the permanent cereal plots at Rothamsted provide a strong factual case for continuous cropping, and in this controversy these results are often instanced. Nevertheless, we might as well bear it in mind that these Rothamsted experiments were started long ago not so much to establish a case for continuous cropping as to isolate and measure specific fertilizer effects. The continuous cropping policy was only a necessary means toward an end. To say this, of course, does not alter the facts, and the facts are that at Rothamsted they have plots that have been *continuous monoculture* plots for over a century, and those that are properly treated with fertilizers are still cropping very satisfactorily. We cannot doubt the facts; we can reflect upon the interpretations.

We have then these possible benefits from chemical nitrogen.

1. *General:* Whenever it is known or indicated by experience that the rate of supply of active nitrogen by the soil is too small, we can compensate for this by adding nitrogen in active form.

2. *Particular:* We can balance the loss of total nitrogen from the soil by using chemical nitrogen to reduce the amount and duration of nitrogen restoration, and at the same time, crop rotation methods or occasional leys with mixed farming.

3. *Extreme:* We can balance the loss of total nitrogen with

chemical supplies so sufficiently that we can afford to abandon traditional methods, and adopt continuous or nearly continuous methods of cropping.

There should, however, be a footnote to these benefits. It is one that must be only briefly mentioned now for it will be discussed in more detail when the misuses of fertilizers are considered. All nitrogenous applications must be balanced with sufficient amounts of phosphorus and potassium, either those in the soil or, if these are not enough, by fertilizer application. Additional yields gained by the application of a nitrogenous fertilizer alone cannot be credited to nitrogen entirely; for the extra amount of leaf growth, etc., will force the plant also to draw more phosphorus and potassium from the soil; so that, after harvesting, the soil has lost more phosphorus and potassium than it would have lost had the nitrogen not been applied. This factor has been left out in this discussion about nitrogen because it was clearer to stick to the main point. However, it is, in the total discussion, an important consideration.

How does the use of chemical nitrogen work out in practice? Does it balance this net nitrogen loss? Here is an indication from America, a mass-indication for American farming as a whole. In 1936, Dr. Lipman, a research biochemist of the United States Department of Agriculture, drew up a balance sheet for nitrogen. His figures were calculated from data available, and we must regard them as reasonable indications rather than as accurate measurements.

Losses (by crop removal, grazing, erosion, leaching)	22,899,046 tons
Gains (by fertilizers, manures, rainfall, irrigation waters, seeds, bacteria)	16,253,862 tons
Net annual loss of nitrogen	6,645,184 tons

In the United States virgin soil was cropped year after year without attention to humus maintenance until too late to avoid the consequences; the United States erosion losses of nitrogen

were estimated at 3,500,000 tons, which still leaves a loss from other factors of 3,000,000 tons—and this even when fertilizers have been brought into the credit side of the budget.

It must not be concluded that fertilizers had failed to do the job. The view of American investigators was that fertilizers had been insufficiently used and that husbandry methods to conserve nitrogen had been poor. In 1926–7, the United States consumption of chemical nitrogen for fertilizer purposes was equivalent to 299,200 tons of nitrogen. This being for a year prior to the general depression, we can take it to be a good average measure of annual consumption. The 1935 census in the United States showed that 415 million acres were in some kind of cultivation, but only 161 million acres of these were in reasonable cultivation. So we may reckon that most of the nitrogen purchased was applied to this more active portion of the United States cropland. This gives a *pro rata* figure of *under four pounds of chemical nitrogen per acre*—which is hardly an amount likely to balance the kind of annual loss per acre that we have just seen to be probable.

In Great Britain, the 1926–7 consumption was 40,500 tons of chemical nitrogen. The 1924–30 average arable acreage was about 10 million acres. This shows an average application of about 8 pounds per acre; but it is misleadingly high since it takes no account of any nitrogen used for pastureland to compensate for grazing losses, a frequent practice in that country. This again shows no real attempt on a sufficient scale to use fertilizers to balance the total nitrogen loss.

Even were these figures quadrupled, we should still be some way from a probable balance. We cannot say that fertilizers have failed because the loss still goes on; we can say only that fertilizers have not yet been given any real opportunity to succeed.

This evidence from mass figures can lead to an even more interesting point. In view of the size of the annual loss indicated by America (excluding their special erosion losses), can the amount of chemical nitrogen actually available really cope with the problem? The supply of fertilizer nitrogen comes from three

main sources—nitrate deposits, ammonia by-products, and synthetically *fixed* atmospheric nitrogen. The world production of nitrogen from all these sources was estimated to be as follows: 1926–7, 1.3 million tons; 1927–8, 1.6 million tons; 1930–1, 2.3 million tons. Since in 1936, the United States loss (excluding erosion) was 3 million tons, it is clear that the budget cannot be balanced wholly in this way. The method of chemical fixation of air nitrogen can be expanded, and certainly many more industrial plants of this nature can be set up, but we can hardly reckon upon a development as rapid as would be needed by this world total of nitrogen loss.

The implication is surely this; that no agricultural policy relying upon fertilizers alone to balance nitrogen loss can become prevalent or endure, that the only general policy must be that of combining fertilizer additions with sound conservation practices. In short, diversified farming and ley farming, the efficient use of all farmyard manures, and cover-cropping, must play the fullest possible part in the task. Individual farms that adopt continuous arable practices, relying upon fertilizer nitrogen in the main, are running against the trend of facts, and they can do so in practice only because they are special cases in a minority. For, if very large numbers of farms did so, the world's nitrogen production capacity would not cover their needs, and the general nitrogen decline would be accelerated.

Little mention has been made of the fifth-column bacteria which turn nitrates back into nitrogen, but it is generally considered that these cause little loss except in waterlogged or otherwise non-aerated conditions.

It seems fair to say that from the point of view of the soil there is no true nitrogen cycle in any sense. Rather, it is a spiral, like the economists' inflation spiral, with the nitrogen budget constantly failing to balance itself, loop after loop. To introduce another picture, it is like passing a water supply along in a chain of buckets with the final bucket, the active nitrogen bucket, badly holed. To keep enough water passing along the system, we must try to stop up the holes as much as possible, but we know

this cannot be achieved completely; so we must bring into the final bucket additional supplies, i.e., fertilizers. Suppose instead we filled up the earlier buckets with more water more frequently we used nitrogen buckets which are not holed? In a later chapter this question must be considered: is it possible to balance nitrogen losses in agriculture with organic nitrogen supplies alone?

CHAPTER VI

THE HUMUS MONOPOLY

"Humus is a natural body; it is a composite entity, just as are plant, animal, and microbial substances; it is even more complex chemically, since all these materials contribute to its formation." S. A. Waksman.

"In my view, it is more important to investigate the old, well-tried, organic manures of the farm, and to see whether they contain the plant-growth substances, and in what proportions. If growth-substances are present in organic manures, a knowledge of their presence and distribution may help us to understand more about soil fertility. . . ." Hugh Nicol.

MAN'S ATTEMPTS to establish monopolies are trivial compared with those of Nature. Our monopolies may indeed look formidable for a time, but along comes a substitute material or a new process and something that had seemed set for centuries crumbles in a few years. The monopoly power of humus is much more solid and permanent. We may pretend to flout it, but let pretense last too long and the very soil itself may be swept away by wind and water.

We have already glanced at the properties of humus. In talking more precisely about it, the distinction must be drawn between *organic matter* and *humus*. We must not assume that they are one and the same, or that the former inevitably becomes the latter in the course of time. Certainly the conversion of raw organic matter to humus is a natural change that tends to occur, but it depends upon specific conditions and, if these conditions are not present, it may be a very slow change or it may stop halfway. Too often is it assumed that the addition of raw organic matter is *ipso facto* a restoration of humus.

Unfortunately we are often forced to talk about organic mat-

ter when it would be preferable to talk about humus. It is easier to make an estimate of the organic matter content of a soil than of the humus content. It is not possible to measure the actual contents of either with accuracy, but we can keep on surer ground if we stick to the organic matter. Therefore, in quantitative approaches to the subject, we have to be guided by the organic matter estimate, and we must assume that this is more or less a measure of the humus content *where* we know that the general soil conditions will favor humification, i.e., conversion into humus.

A digressive start by way of ancient history is not without point. Soil is in origin *weathered rock*—no geologist, or pedologist (which only means a scientist who studies geology with special reference to soil), would quarrel with this statement. By a natural process or combination of such processes, rocks break down and become particles; that is how *first of all* a soil forms. Such a soil is composed of minerals. It has no organic content initially. The first plants that grew in these early soils, therefore, had to live on mineral nutrients plus nitrogen from storm rainfall. They lived, flowered, seeded, and died; at death the *organic* remains dropped back upon their mineral bed and thus humus began to enter the soil. In short, *the chemical nutrients came first in history, and humus came after.* Not until many crops had grown and died could there have been in any of these infant soils a significant store of humus.

The supplies of phosphorus and potassium in weathered rock must have been very large in most cases, and a high proportion of these supplies was probably in an active form. Nitrogen is likely to have been the limiting factor. However, storm rainfall and gradually increasing bacterial production must steadily have increased the nitrogen quota, and plant growth must have multiplied correspondingly. However, later—and these rather casual time references must be regarded as covering centuries—the initial stores of phosphorus and potassium in active form may have diminished, and a new limiting factor to plant growth reared its head. Admittedly, the plants' intake of phosphorus and potas-

sium was regularly returned to the soil in the dead remains of each generation, but this must be visualized as a replacement of active nutrients with less active nutrients, locked up in the organic constitution of plants' bodies. We can reasonably assume a steady fall in the favorable proportion of active to inactive nutrients. It is at this stage that the importance of humus would seem to have entered the picture. Humus has the property of holding nutrients, of saving them from loss by leaching or from imprisonment in insoluble mineral complexes. The humus hold, though strong enough to prevent leaching and inactive fixation, is not so strong that nutrients will not be released to plants more easily than from *insoluble* mineral attachments. Plants, finding their soluble nutrients less abundant in the soil, began to evolve with an eye to the next best thing, the nutrients loosely attached to humus. An increasing dependence upon humus, an increasing association of plant roots with humus, developed. Consequently, although humus came second on the historical menu, by a natural process of evolution, its importance rapidly expanded.

This is, of course, purely speculative reconstruction. It constitutes no solid kind of proof for any argument. However, we do know that plants *can* grow upon a chemical nutrient basis alone—this happens wherever hydroponics is practiced. The geologist states quite definitely that these entirely mineral soils—which he calls skeleton soils—supported plant growth and became mineral-plus-humus soils as we know soils today only in the course of time. It is difficult not to believe that the plant's dependence upon humus evolved *after* a dependence upon mineral nutrients.

Of course, this slow natural process resulted in a steady accumulation of humus. Agriculture upset the balance. The extent to which our agricultural actions are wholly artificial is seldom admitted. For her own aims and purposes, Nature had arranged for these organic matter changes to take place infinitely slowly. The soil's surface was covered with a mat of mixed vegetation. This mat prevented any rapid soil aeration. The mat was torn up in the first operations of agriculture and air rushed into the

soil. The bacteria turning organic matter into humus had been restricted in their activity and growth by the limited oxygen supply; the inrush of air stimulated this bacterial furnace. Organic matter turned into humus faster and faster, and humus itself was consumed more quickly as bacterial fuel. Agriculture thus brought about two fundamental changes; first, a big jump in the rate of change and consumption of organic matter; second, a big drop in the rate of supply of fresh organic matter—for, instead of a thickly growing mixed crop continually returning itself to the soil, there was a spaced crop of which only a part at the most was returned.

This did not happen only in the beginnings of agriculture. It happened, and indeed happens now, wherever and whenever virgin soil is taken for cultivation. It happened some centuries ago in central and south-eastern Europe; and not so long ago in America and Canada. The pioneers fell upon rich virgin soils with fertility slowly built up during many centuries, and they cropped them until they had so unbalanced these arrangements in a short time that either they had to find new virgin soil to rape or accept a dwindling, struggling rate of cropping if they stayed on the scene of their crimes. Wherever nothing is done to compensate for the unbalancing of the natural conditions of virgin fertility, wherever crops are taken without an attempt to replace the consumed fertility, exhaustion rapidly succeeds exploitation. Erosion is simply a polite word for the final stage of the felony. Soil fertility, however, has disappeared before erosion turns up. It is not necessary to go so far down the path of disaster as erosion to realize that agriculture must arrange to compensate for the changes that it imposes upon Nature.

How do quantitative measurements square with these opinions? What is accurately known about the decline of organic matter content through agriculture? An American measurement comes from Missouri. Here scientists found side by side plots of natural untouched prairie land and plots of long-cultivated arable land. The latter had been growing cereals continuously

for 60 years, the crop stubble being the only regular source of fresh organic matter. It is reasonable to assume that the arable soil would have been the same as the adjoining prairie land had it not been requisitioned by agriculture. Soil analysis showed that the organic matter content of the cereal soil was 38 per cent less than that of the prairie soil. In 60 years of arable cropping, 38 per cent of the organic matter, accumulated in centuries, had been lost.

Another measurement of this kind is based upon the soil's capacity to produce the nitrate form of nitrogen. In the last chapter we saw that nitrate production in the soil depends upon bacterial activity, and this in turn depends upon a supply of organic matter. It is, therefore, a fairly sound assumption to say that the capacity of a soil to provide nitrate-nitrogen is a measure of its organic content. Of course, the bacterial activity depends upon other factors too, such as temperature, acidity, moisture, etc., and it will vary from day to day. If regular analyses of the soil for nitrates are carried out, and the figures plotted on a graph, the level—or, to put it crudely, the general height of the graph line—will show the soil's nitrifying capacity. Checking this nitrate figure at regular and frequent intervals over 13 years of continuous corn cropping showed that the level of nitrate production in this period dropped by 35 per cent. That is to say, the maxima and minima during the thirteenth year were 35 per cent lower than in the initial year. We cannot conclude that this means there was also a 35 per cent reduction in the organic matter or humus content, but we can safely consider that a substantial reduction of this scale had occurred.

In these two cases, nothing had been put back into the soil except the unwanted crop residues—the root systems and the stubble after harvesting. At Woburn, continuous cereal cropping from 1876 to 1926 showed a 33 per cent loss in organic matter content where only fertilizers had been used, but little loss where large applications of farmyard manure had been regularly given. This comparison was worked out from the figures for car-

bon content and nitrogen content in 1876 and in 1926, it being assumed that the organic matter content was fairly proportional to these figures. These were the figures:

	Carbon Content		*Nitrogen Content*	
	1876	*1926*	*1876*	*1926*
	per cent	*per cent*	*per cent*	*per cent*
Manured plot	1.48	1.5	0.155	0.15
NPK plot	1.48	1.0	0.155	0.09

As an interesting digression, note that in both plots the ratio of carbon to nitrogen (C */N) in 1876 and in 1926 remains at about 10:1—the general ratio for humus. This demonstrates, as do so many numerical measurements, that the equilibrium between C and N to which soils tend to arrive is expressed by this steady ratio of roughly 10:1, even though the soil treatments may be very different. It should be borne in mind that this 10:1 figure holds good for our kind of climate, a different ratio may hold good for tropical climates, etc.

These Woburn figures would seem at first sight to be rather damaging to the case for fertilizers. Here is a clear indication that the continued use of chemicals does not maintain organic matter content but that the continued use of manure does. And, since it has been seen in other tests that fertility declines with decreases in this organic content, it is a fair argument to suppose that chemicals cannot maintain cropping capacity indefinitely. However, the reverse has been proved at Rothamsted where chemicals have maintained a high cropping level for more than one hundred years. Nevertheless, take the Woburn figures at their face value—regard them as the only figures. To conclude that we should use manures only is not a constructive conclusion having regard to the limited supplies of manures that are available and to the heavy cropping needs. The constructive conclusion to be drawn is surely this—if we are going to use fertilizers regularly, we must also take other steps to maintain the losing balance of organic matter. In short, manures and fertilizers are comple-

* C = carbon.

mentary, NPK supplies and humus supplies must *both* be provided.

The very pro-fertilizer advocate can rightly argue that it has not been proved that humus is so essential to cropping, that it has not been shown that this decline in organic content really affects the issue seriously. The Rothamsted experiments with the continuous fertilizer application and use of the same cereal plots and the new ventures of hydroponics certainly point to a rather less important role for humus. However, surely it is impossible to ignore the various virtues of humus. It acts as a larder for nutrients. It provides an energy supply for bacteria, especially for those that operate as nitrogen producers or nitrogen brokers. It binds light soils together and opens up heavy soils. In dry weather it holds moisture. And, though opinion is divided as to the importance of soil fungi in fertility maintenance, humus is essential to fungal development, and, if we cannot assess this value with certainty, at least we know it to have some positive value.

Besides these well-known and well-emphasized functions, there are two others that would seem to have most direct connections with fertility. One is so obvious that it is usually taken for granted; the other is apparent only in the light of recent research. Taking the obvious one first: the bacterial consumption of humus or organic matter as energy food means that the carbonaceous matter, the cellulose, etc., is oxidized, that is to say, is turned into carbon dioxide, just as we utilize sugar and starch for our energy foods. This carbon dioxide is produced *in the soil.* Now, whether carbon dioxide is in the soil solution or in the air round about the soil, it is essential to plant development. In the air, it is absorbed by the plant leaf and turned into higher carbon compounds by photosynthesis. In the soil solution, it is believed to form the extracting solvent for locked-up nutrients. The humus source of carbon dioxide can therefore be regarded as doubly vital to plant growth. Of course, there are other sources of carbon dioxide. We ourselves exhale it, chimneys and flues exhaust it everywhere that fuel is burnt. Lime burning kilns also

send off large quantities. The amount provided by humus consumption must surely be one of the major sources of carbon dioxide found in the air, and moreover it is a source that provides carbon dioxide in the right place, at the right time. Carbon dioxide is a heavy gas which does not diffuse rapidly. This is shown by the fact that the carbon dioxide content of air in towns and cities, where there are many more chimneys and pairs of lungs, will always be higher than that of sparsely populated and non-industrialized localities. In an agricultural area, a sharp decline in the humus content of soils could bring about a decline in the carbon dioxide content of both the soil solution and the air above the soil, a decline that would be only partly and slowly balanced by carbon dioxide production in urban areas.

So far as the value of carbon dioxide in the soil solution is concerned, it should be said that the original theory of the Dyer solution, based upon the idea that plant-saps extracted the nutrients from the soil's store, is no longer generally accepted. Instead, the extracting is believed to be due to carbonic acid, the acid formed, when carbon dioxide combines with water. We know that carbonic acid, although a weak acid, is strong enough to extract phosphorus and potassium slowly from the kind of complexes these nutrients form in the soil, but there is no evidence, as Hall pointed out, that plant juices ever operate outside their own root-cells. So here again the presence of an adequate amount of carbon dioxide in the soil seems vital to fertility, and it is hardly an unreasonable assumption to say that this adequacy depends in part upon humus or organic matter content.

The other notable function of humus is connected with what are now called *plant-growth substances.* It has been shown in recent years that certain rather complex organic chemicals, if applied in very dilute solutions as dressings to roots and even stems of plants, stimulate or induce root development. The root development of seedlings or cuttings can be accelerated. Roots can even be made to grow on stems where normally roots would not be expected at all. The chemical names of these substances do not matter very much to this argument; indole-acetic acid,

naphthyl-acetic acid, indole-proprionic acid, these are some of the substances. What does matter is that these substances very probably occur naturally as trace-components of humus. We have to limit the argument with this *very probably* because the complete chemistry of the *organic matter humus* change is so far only partially understood. It is much more complicated than the simple changes that occur in test tubes according to the textbook rules of the chemical road. Bacteria, so to speak, do not always keep the left, they cause unorthodox reactions. However, from the known reactions that take place and from those which are thought to take place, chemists have drawn up a kind of blueprint of the complicated conversion of raw organic matter into humus—and, in this shape of things that probably happen, many of these plant-growth substances turn up as components of the various reactions. So we have every reason to presume that this root-inducing property of these particular substances, though apparently new and artificial in the laboratory, is quite old and essential in Nature's processes.

Modern research has shown that minute doses of these chemicals will make roots grow. This discovery has been put upon a commercial basis and preparations of these chemicals are widely sold for application to cuttings and seedlings. Why regard this development as a luxury development—as something additional to Nature's mechanisms and strategy? It is reasonable to suppose that these substances perform their special functions, and always have done so, in the soil, that they are always present in the soil as minor components of the humus accumulation. As yet, this is only a theory. It has still to be shown that these substances can be isolated from humus or from changing organic matter. It has then to be proved that traces of them are essential, and not just helpful, to healthy root development. If further research settles these points, we have then an enormous piece of evidence for the indispensability of humus.

All this seems enough for the argument that humus is an essential member of the soil fertility system. We cannot expect to maintain fertility without humus, we must make a continual

effort to increase or preserve the humus level in a soil. Anybody who is still not convinced about the essentialness of humus should air his skepticism by reading some of the books that are devoted exclusively to humus considerations.

However, a problem is not settled by declaring that some particular thing is essential. Money is an essential of life, but most of our writing and talking about money is concerned not with its essentialness but with ways and means of getting it and keeping it. It is the same with humus. The problem is not so much why or how is it vital, but how to get enough. The next step, then, is to consider the practical problem of humus supply.

The problem has the subdivisions: first, supply of raw organic matter, second, conversion of this into humus. It must be remembered that the ratio C/N * in humus is about 10:1 whereas most organic matter in raw state has a ratio of 33:1 or even 50:1. Bacteria may be able to bring off *googlie* chemical reactions, but they cannot violate the *quantitative* laws. A bulk of matter of C/N ratio 50:1 cannot change into a similar bulk of humus of C/N ratio 10:1, unless additional nitrogen is secured from other sources, the maximum amount of humus obtainable will be equivalent to one-fifth of the original carbonaceous matter. If there are 50 parts of carbon to 1 of nitrogen in the raw matter, 10 parts will go into the humus with the 1 part of nitrogen; the rest must be lost as carbon dioxide. This is not always realized. Growers dig in great bulks of fresh organic matter and pat themselves on the back for their liberal attention to the humus problem, but the humus actually provided is very much less than they wishfully imagine.

Of course, a great deal of organic matter is bound to be burned away to carbon dioxide when the bacteria and other organisms use it as energy food. They consume things like celluloses just as we consume sugars and starches, the oxidation processes of digestion providing energy. The nitrogen is not just needed as a kind of yard-stick for the 10-times-as-much carbon that can be brought into the humus complex. It must not be thought of simply as an

* Carbon to nitrogen.

inactive partner in the process. The bacteria need a nitrogen food supply just as plants need nitrogen, and the more nitrogen they have the more humus conversion they will bring about. The 10:1 C/N ratio is best regarded as an indirect measurement of their nitrogen needs.

When organic matter is dug into the soil, the bacteria will actually take nitrogen from the soil in order to bring about a better C/N balance for their purposes. This nitrogen is not lost to the soil for eventually it is again released by the bacteria in the humus, but *temporarily* there is a substantial reduction in the quota of nitrogen available for plant needs. This immediate effect of lowered fertility is often experienced to a marked degree when highly carbonaceous matter like straw is dug into soil.

Another factor that should not be forgotten when considering how much humus is likely to be provided by a quantity of organic matter is the moisture content. Most raw organic matter has a water content of about 90 per cent or even more. A better idea of this is usually obtained by looking at the water content the other way round. Thus, consider two similar materials of 90 per cent and 80 per cent water content. The dry matter content is respectively 10 per cent and 20 per cent. In thinking about applications of manures of humus-type, therefore, it should not be forgotten that much of the total weight may be just water; and, though this is of value, it is after all the dry matter content that is of most bio-chemical value to fertility. The bacteria would say so anyway.

With these points in mind, the main sources of humus-producing materials can be considered. The best known, of course, is *farmyard manure,* which will be denoted by the letters *F.Y.M.* from here onwards. For hundreds of years F.Y.M. has been the principal humus provider. As already shown in test figures, where F.Y.M. was regularly applied in heavy amount, the organic content of a soil was maintained despite continuous monoculture.

The traditional procedure in making F.Y.M. is to mix both the liquid and solid excretions, using a retaining litter such as straw.

From time to time, this mixture is removed from the shed and piled in a heap. Aided particularly by the active soluble nitrogen in the urine, the bacteria proceed to decompose the mixture. The heap warms, moisture steams off, and eventually there is obtained a friable humus-like product that can be applied to the soil. Now whether this method provides the maximum possible amount of humus or not is largely a matter of chance. It is also a matter of chance whether all the NPK nutrients are preserved to the maximum possible amounts.

It may be felt that efficiency depends upon the initial nitrogen value. However, there is a grave handicap to the nitrogen. The nitrogen in the urine is largely in a very unstable form which is rapidly—indeed, almost immediately—oxidized to ammonium carbonate. And this substance is even more unstable, for it quickly breaks up into free ammonia and free carbon dioxide, which are then lost to the air. This nitrogen loss takes place at appreciable rate in the cattle sheds. The more loosely packed the litter and excrement mixture, the more air gets at the urine nitrogen and the more oxidation to the unstable ammonium carbonate takes place. Great care is needed to minimize this loss. Frequent removal from the floor of the shed to the less air exposed heap, and the encouragement of the animals to trample the litter into a compact state, these are two methods whereby reduction in loss may be secured. However, some loss is inevitable.

This is not all the nitrogen loss associated with F.Y.M. making. In the heap, exposure to rain will cause leaching, especially since much of the nitrogen is in soluble forms. Also, loss of much of the urine by poor litter arrangements or by drainage loss from the sheds is a common feature of average farming practice. The nitrogen loss associated with F.Y.M. making was summarized by Hall as follows: inevitable loss where all possible care is taken: at least 13 per cent, usual loss in practice: 30 per cent to 40 per cent.

There is a further consideration. Cattle consume, say, 100 units of organic matter as fodder. They utilize 50 units for their own energy needs, all the carbon here being lost as carbon dioxide.

Twenty-five units are then lost as carbon dioxide in the bacterial decomposition processes of the manure heap. The remaining 25 units are available for return to the soil as humus. I would stress the point that I am not seeking to deflate the obvious value of F.Y.M. in order to intensify any case for fertilizers. My aim in bringing forward these quantitative estimates is to demonstrate the fact that a very large weight of raw organic matter is needed to produce a moderate amount of humus. There is no quantitative relation that we can express as: ORGANIC MATTER = HUMUS. We can only say: ORGANIC MATTER → HUMUS, and the arrow covers a large amount of loss.

This loss does not matter as much as similar losses of NPK nutrients. Organic matter is not so limited in supply. It is produced in plant growth by the absorption of carbon dioxide and the turning of this carbon into carbohydrates, celluloses, etc. So long as it is realized that there is such a loss, so large a difference between amount of humus and amount of initial organic matter, the magnitude of our needs for supplies of raw organic matter will be more accurately visualized.

This perhaps seems to conflict with the view that F.Y.M. safely maintained the humus content of the soil in *the good old days* of agriculture. To begin with, nobody really knows whether it did or not, for in those good old days these things were not often measured even roughly. If the humus level was better maintained than today, it was not done by F.Y.M. alone. Diversified farming, mixed cropping, alternate leys, the digging in of crops, these other elements of good husbandry all played their parts in addition to much less intensive cropping per acre than the cropping we regard as normal today.

The army was not mechanized—from its stables there came considerable supplies of manure. Transport was dominated by the horse, and here too was an enormous source of animal manure. The times, too, were times of gross meat eating, and ships did not have refrigerator-holds to bring in large quantities of imported meat. The heavy quota of meat per head was home-produced, and there was therefore a much greater proportion of

farm cattle in relation to the population of the country. Townsmen wishing to display an interest in agriculture frequently assert that *what is wanted, is a return to the old days of plenty of farmyard manure!* This view is no doubt colored by the sad fact that they cannot obtain a load or so for their own roses and vegetables. There is no going back. The townsman is unlikely to accept a stagecoach in place of his electric eighty-forty to the city; his wife is equally unlikely to accept the extra housekeeping costs and worries that would result from any attempt to limit rates of cropping to the amounts of animal manure available in modern civilization. In short, we must handle what F.Y.M. we have with the maximum care and make the best use of it in applications, and must try to solve the problem of total humus supply by other means as well.

The human animal is much less useful to soil fertility. For him there is no litter bed. He is a consumer of fertility constantly adopting an irresponsible attitude toward the return of his wastage to the soil. The toilet and the universal sewage pipeline sweep away all that he might put back in thanks for the food he has received. Sanitation, not humus, is the god. Human excretions are just as important as animal excretions, they are organic matter taken from the soil. Modern civilization has erred badly in thinking always of sewage disposal, never of sewage reclamation. City and town all over the country aim at the most speedy and secretive conduction of all this useful organic matter to the nearest river or sea. Engineers have been called upon to tackle the problem from one angle only: inoffensive handling at the lowest cost to the taxpayer. Sewage works indeed abound. One of the triumphs of the sewage engineer has been to cleanse the liquid sewage so efficiently that it can be used as water once again. All this ingenuity devoted to the water—so little to the rest!

Let there be no misunderstanding. This is no suggestion that we should go in for individual salvage, saving our excrements from the sewage systems and returning them to our own little

pieces of garden. What is suggested, and it is not at all original, is that all sewage works should concentrate upon methods of disposal that are also methods of reclamation. And this does not mean the old gambit of the sewage farm, which is at best only a feeble attempt to tackle a big task.

A glance at the usual sewage farm will indicate the size of the problem. The sewage—which should always be visualized as very liquid because of the many ways in which water is added to the matter discharged into sewage lines—is irrigated over light soils. These soils absorb the nutrients and collect the organic matter. However, there have to be many such fields of soil, because irrigation processes cannot go on indefinitely. A field becomes overcharged and underaerated, and it must be rested and exposed or the putrefying bacteria will take charge; and, quite apart from the loss of potential fertility involved, the adjoining district will at once write angry letters to the council about the smell. Then again there is little point in all this salvage of nutrients and organic matter unless crops are grown so that each field has a period of cultivation. Thus, a very large area is needed to handle any sort of flow of sewage, and it has been calculated that an acre of farmland on a sewage farm is needed per 100 persons. This makes the method hopelessly uneconomic for cities or towns of any size, for no council or corporation could set aside an acre per 100 inhabitants on the outskirts of its valuable land. The price of land is such that only the most intensive market-gardening kind of growing can possibly be carried on within urban areas, and a sewage farm cannot even be intensive. This is perhaps begging the question that nationally desirable matters ought not to be considered in the light of whether they pay or not; but the sewage farm method is so cumbersome that it would be ruled out even by the most uneconomic new-world economists.

However, the sewage farm method is rather de-moded anyway, and I have stopped to discuss it only to dissuade the possible opinion that it is the kind of thing we should look to. Our refined sense of sanitation has actually led to quite useful methods

of reclamation almost by accident, certainly without much design. Disposal into rivers and seas led to pollution troubles—rivers were fouled and coastal resorts offended. Sewage engineers had to make the outpourings less noticeable. The general method adopted was settlement in tanks. Much of the solid matter in sewage is in a *mock-solution* state known as a colloidal solution, which is a suspension of such fineness that it seems to be a true solution. Now these colloidal suspensions can be coagulated by the addition of certain chemicals, such as lime or alum. The fine particles then start sticking together in lumps and they sink to the bottom. When this is done with sewage, the colloidal precipitate as it forms also drags down with it a very high proportion of the floating and fluffy organic matter; so the sediment in the tanks is a mixture of the precipitating agent (often lime), of the colloidal mineral matter, and the more bulky organic matter. This mixture is known as sewage sludge. It clearly cannot contain any appreciable amount of *soluble* materials for there is nothing in this process to encourage the precipitation of substances dissolved in the liquid part of the sewage; therefore, we cannot expect sewage sludge to contain any active nutrients, but only the insoluble phosphates and insoluble complex forms of nitrogen.

However, the mistake of the past has been to look at sludge in terms of active chemical nutrients alone, and to forget that it is a good source of organic matter. It has been far too readily dismissed as a valueless or almost valueless fertilizer, and thereby given little consideration as a manure. Practical aspects have also handicapped its development. The sludge is usually very wet, resembling the kind of mud found at river mouths where there is no sand and little shingle. It holds water tenaciously in this state of slime so that drying is not easy. Exposure in thin strips is needed. Exposure in bulk will bring about only a surface dryness except in a long period of time and in warm, drying weather. Yet for distribution to farms some reasonable state of dryness is essential.

The sludge belongs, of course, to the councils responsible for

sanitation. The undertaking of further expense to prepare the sludge in a sufficiently dry condition for agricultural use is a matter for each council of each sewage works to decide. To realize that this further expense might be justified by later results needs not only technical knowledge but faith, and these forms of enterprise are not usually prevalent in municipal councils or committees. On the whole, sludge has been disposed of by dumping, burning, or other means, and the further work involved in producing an agricultural manure has not been very often attempted. The fact that sewage sludge has been treated contemptuously from an NPK angle by many agricultural authorities, and the fact that further processing is needed to make it physically suitable as a manure—these considerations together have stalemated any development in a general sense. Only a few works and councils have displayed initiative, and their example, though successful, has not been widely followed.

The Institute of Sewage Purification has recently issued several reports on this subject, suggesting three methods of getting the sludge to the farms: drying it, mixing it with other surpluses such as straw and composting the mixture, and pumping wet sewage sludge direct to farmland.

Some personal observations may be added to this discussion. One city, in the center of five important agricultural counties, pumps all its sewage into a river near a sea, but the outlying rural district councils each have sewage works. In peacetime, even at the one works where there is some chance of reasonably dry sludge being produced, the demand by farmers is limited to a peak demand in dry weather. In wartime, owing to the shortage of many organic fertilizers and manures, there has been a temporary increase in demand sometimes showing itself in lines of carts and trucks. At other works, where dryness of product is rare, the sludge, even in wartime, is still given away to any farmer who bothers to collect at his own expense and there are no waiting lines. This hardly indicates an appreciative demand even at a time when the soil needs all the help it can get to meet cropping pressure.

Buckwheat pot cultures. The same amount of nitrogen and phosphoric acid in all the pots, but, from left to right, (1) no potash added, (2) single dose of potash added, (3) double dose, (4) triple dose, and (5) twice the triple dose. Note that the amount of foliage is about the same despite other differences because the amount of nitrogen available has been constant.
(Courtesy of Rothamsted Experimental Station.)

So, as a hard fact, and whether we like it or not, we must conclude that at present the human species of animal is throwing away practically all the organic matter consumed. This wastage represents an enormous opportunity for organic-matter reclamation.

A third source of organic matter is crop residues. These can be divided into two kinds: crop residues left in the field at harvesting, e.g., cereal stubble, root systems, etc., and wastages that are separated from the crop at later stages of handling. The former kind of wastage goes back into the soil almost without exception because it is already there and it is easier to dig it in than to dig it out. We had better forget the other kind of wastage for the moment because it needs a section to itself.

The C/N ratio of these residues will never be as favorable as 10:1, and if the residues are to be effectively turned into humus a temporary demand upon soil nitrogen must be made. Only when the soil is richly nitrogenous will the humus-making proceed without temporarily robbing plant life of much of its nitrogen supply. Though this nitrogen is later returned as complex nitrogen in the humus, it can be an awkward factor in agricultural arrangements; for, as seen in the previous chapter, nitrogen shortage rather than nitrogen sufficiency is the general rule in arable farming. It would be very convenient if the borrowing of nitrogen by bacteria took place at times when plants did not want nitrogen, when the ground was bare after harvesting, when —as we have seen—the leaching loss tends to be severe. However, this is not the case for the bacteria tend to be inactive in cold weather or in very wet weather. They make then only small demands for nitrogen; which means they will stop very little soluble nitrogen from being washed away. It is a pity that the leaching loss cannot be prevented by the digging in of raw organic matter. For that might transfigure the whole nitrogen problem. However, it just does not happen. Bacteria and plants like the same conditions and when the soil is most favorable for bacterial activity it is also suitable for active plant-growth. Bacteria being more mobile than plants, they are more likely to satisfy

their nutrient needs to the full when there is not enough active nutrient in the soil for both.

It is sometimes argued that leguminous crop residues will turn into humus *on their own steam* because of the nitrogen they have themselves produced. This is a dubious argument. Certainly the legume has produced most of its own nitrogen during growth, but it has used this nitrogen, and it has—by photosynthesis—brought in a high proportion of carbon to accompany this nitrogen in the plant structure material. Therefore, a legume residue is much the same as any other residue—it consists in a large amount of carbon associated with a small amount of nitrogen. The C/N ratio might be a little lower but it is still a big ratio. The fact that this nitrogen has previously been taken from the air by the leguminous bacteria makes no subsequent difference to the problem of decomposition. The only special virtue that may be possessed by legume residues depends upon a continued activity of the nitrogen-fixing bacteria colonies after the harvesting of the crop. These colonies may continue to operate for a short time, but this is unlikely to persist for long after the death of the host plant; so the possibility of selfsufficient humus production via legumes must not be overestimated.

Green manuring, that is, the growth of crops solely for addition to the soil to increase organic matter, is not a very popular practice. Research has often led to doubtful conclusions as to its benefits. Taking a general view of scientific recommendations, it would seem to be the research centers of the Southern and Eastern United States that press for green manuring most strongly. In Britain the practice is usually described in a somewhat negative manner. Thus H. G. Sanders in his *Outline of British Crop Husbandry* says: "Experiments at Woburn and elsewhere have not shown green manuring in a very favorable light, and it certainly seems that the benefits accruing from it are insufficient to justify the sacrifice of a crop. This view conforms with general practice, in which crops grown for green manuring are usually in the nature of catch crops." Later the same authority says: ". . . although there are to be found some farmers who are en-

thusiastic . . . the general practical view is that its utility is not great, and this is supported by the available experimental evidence." On the other hand we have A. J. Pieters and R. McKee of the United States Bureau of Plant Industry observing in a United States official publication, *Soils and Men:* "Green manure crops are used in the expectation that the yields of subsequent crops will be increased, and this hope is commonly realized."

Anybody looking at both English and American advice on methods of fertility maintenance cannot fail to notice that green manuring is regarded rather coldly here and rather warmly there. We can deduce quite an interesting point from this difference. In America, arable farming has been large-scale continuous monoculture with little mixed farming or rotation of crop. The organic matter contents of the once rich soils has run down to a far more serious extent than in Britain. Therefore under those conditions green manuring has shown the better results. Or, looked at in a slightly different way, it depends *how badly* a soil needs organic matter whether green manuring is likely to give results that are worth the sacrifice of a more immediately profitable crop. A *small* addition of humus will be more important to very humus-deficient soil than to soil which is still reasonably humus-provided.

The suggestion in the last paragraph that the humus from green manuring is only *small* should be explained. A green manure crop provides, say, one ton per acre of dry matter. Suppose 50 per cent of this becomes humus—a generous estimate of probability. Thus about 1,000 pounds of humus is added to the soil. Even a mere 1 per cent of humus in the top-soil of an acre would weigh 20,000 pounds, so the addition from the green manure crop is only 1/20th of 1 per cent, which is not a great amount.

Even the United States enthusiasts for green-manuring practice go no farther than to say it is a method of humus *maintenance.* They do not claim that it can very much *increase* humus content during arable cropping. If this smallness of effect is true where an entire crop is dug in, how much smaller must be the effect of

the digging-in of crop residues, stubbles, root systems, etc. We are not justified in assuming that the organic matter content of soil can be *safely* maintained in a system of continuous arable cultivation with humus additions that are derived only from crop residues and occasional green-manure crops.

It can always be argued against this kind of conclusion that one hundred years of continuous cereal cropping at Rothamsted has been successful even on plots where fertilizers and not manures have been used. However, the Woburn figures show a 33 and 1/3 per cent reduction in organic content over fifty years of such cropping against no loss where organic matter has been added as F.Y.M. The soil at Rothamsted is heavy, while the soil at Woburn is light. Though it has been said in this chapter that humus has no substitute, it is a fact that some of the properties of humus can be exercised by clay. Clay will hold nutrients and moisture where light soils will tend to lose both very easily. The effect of humus shortage, so far as these important properties are concerned, is therefore likely to be much more marked in light soils. To this extent, then, the Rothamsted evidence must be accepted with caution. Also, it must be remembered that the Rothamsted plots are comparatively small. Of necessity they have had to be small for the purposes of investigation and accurate control. The deterioration of a soil through decrease in organic matter content is less likely to show up quickly on a small plot than on a very large area. Erosion and similar soil-deteriorating effects are large-scale matters, and in an area that is laid out in plots or strips of different types of cultivation, etc., single plots are protected from wind and water damage by less vulnerable adjacent plots. Just how much that argument can fairly be used against the Rothamsted evidence probably nobody could decide. However, it is an argument that must be considered, especially as the Rothamsted experiment proceeds.

While we are on this point, we must consider the possibility that humus-content might be raised indirectly by the application of chemical fertilizers. We have no right to assume that, because the fertilizers will cause extra cropping, there will actually be a

greater drain upon humus, and moreover one that will be uncompensated. This is a frequently stated argument, but whether it holds good or not rests not upon ideas but upon measured facts. After all, on the income side of the budget, the crop residues will be greater, and also the nitrogen content of the soil for bacterial stimulation is likely to be greater so that organic matter will more quickly be turned into effective humus.

Here are the carbon contents of soils after 70 years of different and continuous fertilizer treatments:

	No Nitrogen Supply	*Sulfate of Ammonia*		*Nitrate of Soda*	
		single dose	*double dose*	*single dose*	*double dose*
Carbon per cent in soil	1.15	1.14	1.41	1.52	1.73

These figures show that, apart from the single dose treatment with sulfate of ammonia, organic matter has been increased by the use of chemical nitrogen. We must be careful not to regard this increase as too conclusive for the comparison is against a plot where cropping has been conducted without nitrogen additions, and not against a plot where F.Y.M. has been added. The comparison does show that the use of a *sufficiency* of chemical nitrogen holds up the organic content better than the use of little or none.

This is an interesting example of the value of actual measurements. Most advocates of exclusively manurial practice invariably assume that all fertilizer applications must destroy organic matter at a high and uncompensated rate. Here is solid factual evidence which shows that different fertilizer treatments give very different rates of change, and that the loss is less for larger applications because these larger applications produce greater compensating effects. It is unwise to lay down laws for fertility changes on theoretical ideas alone, assumptions must be checked by factual determinations.

However, this must not lead to the idea that organic matter can be maintained by fertilizer applications alone. We have al-

ready seen plenty of evidence showing that it is maintained more efficiently by natural manures. Crop residue and green-manure sources are means by which fertilizers can indirectly provide organic matter, but a general survey of the evidence leads to the conclusion that these methods will not always be enough. What other sources of organic matter can be tapped? For, however much F.Y.M. and animal manures may be praised, we have got to be practical and realize that the supply is short.

This brings us to a wide range of organic wastes other than the crop wastages left on the fields and dug in. It is here that the composting idea comes into its own. Few people today will be without some idea of what composting is. The process has been much publicized, many books and articles have been devoted entirely to it. Claims of the widest nature have been made for compost manures. It is very difficult for anybody to predict just how great a revolution in our fertility practices composting may induce, but in this chapter and the next we must try to arrive at a reasonable estimate. The school of thought that indicts fertilizers plays the compost manure as its ace of trumps, and certainly no other organic matter source would seem more likely than compost to balance the difference between F.Y.M. supply and actual soil needs.

The basic idea behind composting is simple enough. The raw organic matter is bacterially decomposed outside the soil, an imitation of soil conditions being arranged in the heap or pit. Those conditions are: aeration, control of acidity, addition of moisture (not sodden wetness), warmth, and nitrogenous food for the bacteria to balance the high C/N ratio in the raw materials. The various schemes for composting are based upon the provision of these favorable conditions. The great advantage of the compost heap over decomposition within the soil is that the heap can preserve even in winter very much higher temperatures, so that the speed of humus production is much accelerated. Raw organic matter in the soil will be very slowly converted in cold weather since the loss of heat from small separated integrities is great, but, if all the organic matter is packed together in a heap

exposing only a small proportion of surface to cooling influences, temperature fall is minimized and bacterial activity can proceed at a better rate.

The other factors, however, need special care. A heap tends to resist aeration. It packs tight under its own weight, and this is much more marked in very wet weather. In arable soil continued aeration is more likely to be maintained. The acid tendencies of bacterial decomposition in the soil will, unless the soil itself is acid, be balanced by the lime in the soil. This is not the case in the more concentrated conditions of a compost heap, and lime must be added. Similarly, there is no permanent store of moisture for the bacteria to draw upon so that, in dry weather or when dealing with naturally dry wastes such as straw, water must be added to the heap.

The initial stage in compost decomposition is a fungal activity. This can be observed as a gray powdery coating on the organic wastes. The bacterial activity follows this stage.

For practical details, reference should be made to one or two of many excellent books on composting. Lime to correct acidity, turning at intervals to maintain aeration, fertilizer additions to provide the extra nitrogen—these are the basic practical steps though different composting plans vary in details. Sir Albert Howard's Indore system is based upon the joint composting of animal and vegetable wastes, and it is claimed that this method produces the only truly *natural* humus. The method was devised by the comparison with the humus production in the beds of forests where fallen leaves, bird droppings and dead insects and animals speedily decompose together to produce rich humus soil. Nitrogen additions are not so much needed in well-balanced Indore composts for the C/N ratio of mixed animal and vegetable wastes will be much lower than where the initial wastes are predominantly vegetable. The implication of the Indore system is that fresh farmyard manure should not be treated separately, but should be brought into the compost with available vegetable matter. Other composting systems are content to start with predominant proportions of vegetable waste and to obtain the extra

nitrogen from external sources. There is a conflict of opinion as to whether the source should be organic or inorganic, whether sulfate of ammonia produces inferior humus to dried blood, and so on.

This point—organic or inorganic source for the added nitrogen—is not unimportant. It will be best settled, of course, by long-term test, but this kind of investigation must indeed be long-term, for not only must many compost manures be made and compared, but their results in the soil can be fairly compared only over long periods. Meanwhile it must remain a matter of theoretical opinion. Nothing can prove whether organic or inorganic nitrogen suits compost heaps better but detailed research carried out by detached and impartial scientific workers. Until this kind of proof is given, what anybody thinks is no more than a nebulous *hunch.*

There is one method of composting to which considerable publicity has recently been given, and from the point of view of general principles it seems rather the *odd one out.* This method does not add moderate supplies of extra nitrogen but pours in very dilute water solutions of herbs and natural products such as honey. Substantial claims have been made, which would seem to upset all the principles involved in other methods. For it is very difficult to see how material with a C/N ratio of, say, 50:1 can be brought down to humus of C/N at about 10:1 unless there is a high wastage of carbon and therefore of organic matter; for these minute additions of herbs, etc., cannot add very much nitrogen to the mixture. This method needs very considerable quantitative investigation; for, without being dogmatic, one can fairly say that its claims are not compatible with a considerable volume of other evidence now generally accepted.

All crop wastes can be composted, though one of the biggest wastes—straw—presents the difficulty of a very high C/N ratio coupled with a tendency not to retain moisture. The composting of sewage sludge and straw together has already been mentioned. Seaweed and even sawdust are two other wastes which have been successfully incorporated. The great value of the compost

process is that it enables us to bring in these other organic wastes that have not so directly been derived from the soil, and whose raw addition to the soil would present many difficulties. The difficulty of the method lies in the labor involved, in its cumbersome nature. So far it has been adopted more by the intensive market growers than by the farmers. Not only must the heap be built up, it must also be turned once or twice during the period of composting; and these operations must be performed upon a much greater bulk and weight of material than that of the final product. Transport is needed to bring to the farms the bulky raw materials to add to those provided by the farm itself. Composting enthusiasts are apt to dismiss this factor rather too readily, or to describe successes from types of farming where considerable organic wastage is actually produced upon the farm itself. The introduction of mechanical methods of heap-building and heap-turning will greatly reduce the labor problem. As to an announcement in the farming press, a portable crane with self-loading grab, which can take up 2½ to 3 tons of manure in six or seven minutes, has been marketed by a well-known agricultural engineering firm. It is said to be suited to handle sugar-beet, to potato clamping, and to compost turning. From a private source I understand that orders have been given in hundreds and that the machines are in full-scale production.

Clearly no other organic matter source offers so large a chance of humus maintenance and enrichment as the compost manure. If we could compost all the wastes that we today so prolificly discard, the shortage of F.Y.M. would cease to trouble us. However, we must not jump from this happy potentiality to the idea that *ipso facto* we shall balance the NPK budget at the same time. We have been considering the compost manure *as a supplier of humus only*. How much it can correct NPK deficiencies at the same time is an entirely separate matter. Here we can afford to assume nothing. An attempt to estimate the NPK value of compost and other natural manures will be made in the next chapter. There are indeed quite a number of growers who meet all their deficiencies with composts, or who claim to do so by

their results, and they tend to argue that their policy can be made universal. They are composting very large quantities of organic matter wastes and they are at present able to do so only because every grower is not also following the same practice. Whether this could be universal for satisfying NPK and humus needs completely depends upon (a) the total NPK needs of agriculture and horticulture; (b) the NPK value of compost manures, and (c) the total amount of compost manure that can be practicably produced. These points cannot be ignored or brushed aside.

If Sir Albert Howard has been indefatigable as an advocate of composting, Sir George Stapledon has been an equally indefatigable advocate of the ley. And this method is more likely to appeal to most farmers. In the previous chapter it was examined as a method of building up soil nitrogen, but both the turf finally plowed in and all the organic waste that falls back on to the soil during the ley do at the same time make a powerful humus contribution. It is a slower method and it involves diversified farming or alternatively a periodic long sacrifice of arable cropping. However, neither the ley nor the compost heap is necessarily exclusive, and it may be that the regular use of compost manures during arable cropping and ley farming after a long arable period might together maintain organic content at a higher level than is possible by either means alone.

There are at least two other sources of humus to be considered: peat and buried household refuse.

Peat is the product of organic matter rotted or semirotted over hundreds of years in waterlogged or once waterlogged areas. This rotting process has not been the compost kind but the nonaerated kind, carried on by the putrefying kind of bacteria. Nevertheless, when swamps and bogs recede, a great deal of organic matter in various states of decomposition is found. Inasmuch as air may reach the material after the water has subsided or drained away, humus-forming processes may operate. So peat is of humus-type to some extent, though on the whole it must be regarded as organic matter that can be converted into humus

when transferred to conditions that will favor this process. In short, peat is carbonaceous matter. It is not likely to possess more than traces of soluble nitrogen, phosphorus, or potassium, because the water that has covered it for so long has washed these nutrients out. It is also very acid, because all free alkaline material has also been washed out and because the non-aerated bacterial processes have been acid-forming. However, peat must not be ignored. It represents the humus-loss of previous generations, and Nature has conveniently saved it in specific deposits. The artificial drainage of wet areas frequently uncovers rich deposits of peat. Peat is strongly recognized in the United States as a humus-source. In 1912, Americans used 47,000 tons of their own peat; in 1936, they used 46,000 tons of their own and 75,000 tons of imported peat, mainly from Canada, Germany, and Sweden.

The household refuse source is not dissimilar though man and not Nature has created the deposits. Many authorities are faced with the eternal and cumbersome problem of what to do with the contents of dustbins. A large pit is filled up by cartloads of the unsorted refuse, and then sealed with a thick top-covering of soil. And that is that—so far as civilization's further interest in the refuse is concerned. Yet the subsequent course of events in the tip is highly important. To begin with there is enough air sealed with the refuse to allow the usual composting processes to take place, and in such a big heap, so well insulated against heat loss, the temperature rise is very steep indeed, and the bacterial activity most intense. However, the limiting factor is air, and eventually the supply of air is used up; and then the only bacteria that can continue the decay processes are those that can extract oxygen from the substances in the tip. We have then less efficient humus-making with a good deal of putrefying loss of raw materials in gaseous forms. But, losses or no losses, much of the organic matter undergoes change, whether to proper humus in the early stages or to something near to humus in the later airless stages. These tips, if opened up after 2 or 3 years, are likely to be mines of organic matter in a useful condition. With the non-organic matter sorted out mechanically, a humus-type manure

covering the needs of large areas of farmland would be available at low cost. For here, by accident rather than design, the not so distant past has composted some of its wastage. This seems to be a chance for bold enterprise, either public or private.

So much for a survey of our likely sources of humus. Considering them together rather than separately, one point that emerges is the close similarity of farmyard manure stacks and compost heaps. In the farmyard manure stack there is plenty of nitrogenous raw material combined with relatively small quotas of vegetable matter, and in the compost heap, the reverse tendency is likely. Also, one of the problems involved in F.Y.M. manufacture is that the nitrogen is readily lost. If we combine the two practices, not only should we be likely to get a better humus at the end, but the active nitrogen would be quickly seized by the bacteria needing it for their attack upon the vegetable matter, and the wasting tendencies would be minimized. Therefore, it should be a much better proposition for farmers to turn their F.Y.M. heaps into compost heaps by bringing into their farms extra amounts of compostable vegetable waste; better, because they will produce more humus and lose less nitrogen. So, even if the composting habit became a farming habit only to this extent, the soil's humus needs would be more reasonably met. Psychologically, this seems the most likely manner in which to introduce composting into conservative farming. The very fact that in any case there has to be a heap for the normal F.Y.M. practice is a powerful argument in favor of this as a first step in what might be called the technique of process development. For there is more than science and logic in the work of getting new habits and ideas into the regular program of established industries, as anyone who has ever tried to *put over* new processes knows. Not that composting is really novel. It is old as the hills. However, it is comparatively *new* in a revival sense.

Of course, it is very obvious that modern civilization is at present throwing away incredible quantities of potential humus, just as in life the spendthrifts are usually those very people whose financial background is precarious. In drawing such powerful

attention to this *here-today-and-gone-tomorrow* attitude, the humus experts—led by Howard—have performed a very great public service. That they go farther than this, and extend their claims for humus to the view that it must supersede chemical fertilizers altogether, is beside the point; or, at any rate, it is quite another point. Their attack upon fertilizers must be examined later. Sir Albert Howard has been described by Raymond Bush, the fruit expert, as *the father of humus*—a title that Howard would undoubtedly disclaim modestly since his practical work on humus has been based upon a discerning study of the age-old humus processes of Nature. Rather should he be called the modern foster-parent of a neglected child. Today, after much energetic lone fighting, Howard has several lieutenants, notably Lady Eve Balfour, who is both a practitioner of humus farming and a skilled advocate of the humus argument.

This school of thought tends to accuse agricultural scientists of underrating the importance of humus, of overstressing the NPK needs of plants. This is a difficult accusation to prove or to refute. Owing to the comparative novelty of NPK ideas about plant growth—for what is a hundred years in the life of agriculture?—much more attention and research has undoubtedly been devoted to the nutrients, and this has shifted the emphasis more or less accidentally away from humus to NPK. Only a few orthodox scientists have ever stated that humus is of lesser importance. In the early days of fertilizers, there was perhaps this tendency. We see the same kind of short-term tendency in our own time whenever a new antiseptic or disease-fighting drug is presented to us; limitations of new and exciting developments are sometimes not realized until they are forcibly experienced. However, it is quite wrong to suppose that orthodox agricultural science today, or for many years past, fails or has failed to recognize the necessity of humus. As a tail-piece, here is a book by someone with a decidedly chemical mentality, but with one of the longest chapters devoted entirely to humus!

APPENDIX

(1) *Air and Humus*

There seems to be some confusion among humus interpreters about organic matter decomposition with air (aerobic) and without air (anaerobic). They seem to want to have it both ways. While they insist upon aeration as a vital condition in composting for the production of true humus, they frequently describe as humus the products of anaerobic decomposition, e.g., in refuse tips or peat bogs.

I have, throughout this chapter, adopted what is perhaps an unjustified assumption: the view that humus in the true sense comes only from aerobic decompositions, and that the product from airless bacterial activity is not true humus, but something more intermediate, something that can still turn into humus if put into the soil and under aerobic conditions. I have found it difficult to check this assumption, for the humus writers talk about humus in these anaerobic natural deposits as often as they talk about organic matter, when it would seem that all the time they really mean *potential* humus. For it is known that in airless decomposition much nitrogen is lost by the extraction of oxygen from nitrates by bacteria, and if this happens how can a final C/N ratio of 10:1 be attained?

However, it seems better that it should be made clear that this view, expressed several times in the chapter, is a personal assumption, and one to which some humus enthusiasts may object.

The Russian scientist, Gel'tser, e.g., defines humus not as the total aggregate of decomposed organic matter but only as that fraction of it which can combine with the mineral part of the soil to form organo-mineral aggregates. Gel'tser has made enough progress in this work to produce bacterially *synthetic* humus of this kind, which possesses the property of binding the soil particles.

A further line indicated in this work is that this active fraction of the total humus complex is the part derived from the carbohydrates and nitrogenous materials fed upon by the bacteria for

energy—the rest of the complex, derived from other types of organic matter, is non-active.

It seems that the future researches of this Russian school may be of fundamental importance.

(2) *Sawdust as a Compostable Material*

The general attitude toward sawdust has been that it is of little value to the soil, a pest-encourager, etc. I have not hesitated to include it in the list of compost heap materials since Mr. F. C. King, an experienced compost gardener, has incorporated considerable amounts of it in his heaps with success. After all, sawdust is good carbonaceous matter, and if it is *woody* this handicap has been reduced by its reduction to fine particle size. Dr. Rayner, whose work will be mentioned in a later chapter, has carried out experimental work using sawdust composts. Whatever may be the faults of *raw* sawdust if added directly to the soil, it is established that composted sawdust is beneficial.

CHAPTER VII

BALANCE SHEETS FOR THE SOIL

"Most estimates, certainly most public estimates, contain little more than half the expense that will be incurred in connection with an undertaking, and no man can judge such estimates wisely unless he is able to see not only the beginning but the end of the account." SIR ERNEST BENN, *Confessions of a Capitalist.*

THE DIRECT comparison of natural manures and chemical fertilizers cannot indefinitely be avoided. It is a dangerous comparison because in the very making of it there is implied the idea that each is selfsufficient and separate, that it is a matter of choice which one we use. However, this happens to be a frequent point of view, and one of the best ways of dealing with it is to make the comparison in a reasonably accurate, quantitative manner. Also, it is quite true that natural manures supply some NPK with their rich humus offerings, so that it is possible to meet the demands of crops with manures alone. The accurate comparison of manures and fertilizers will give us a realistic idea of the relative amounts required. From one point of view, then, this comparison is likely to be valuable; though from another it is dangerous and misleading.

Suppose we rely upon manures for both humus supply and NPK supply. The cropping level is then limited by the total quantity of NPK nutrients given to the soil, directly or indirectly, by this treatment. Since we have to aim at an agriculture that produces the amount of food we actually need, and not one which sets up a situation in which we eat shared rations of a fixed amount of produce, we must tackle the problem in reverse; we must fix some specific output of cropping and estimate the quantity of NPK nutrients required for its achievement, and

then from this work out the amount of natural manures required. We can then consider whether such a supply is possible either now or in the future—in short, can it, or could it, be done?

It must not be assumed that the same quotas of NPK in natural manures will be required as with chemical fertilizers. On the one hand, increases in the organic matter of the soil will increase both the bacterial activity of soil and the phosphorus and potassium fixing capacity. On the other hand, quite a large part of the natural manures' contents of total nitrogen and phosphorus will be insoluble and inactive. We cannot therefore gear *Nature plus manures* to the same gradient as *Nature plus fertilizers*. In making this comparison, we shall have to adjust our ideas in these respects as we go along. For the total NPK values of these natural manures figures vary considerably according to origin and method of making or handling. Here are a number of figures for F.Y.M.:

	Nitrogen per cent	*Total Phosphoric Acid per cent*	*Potash per cent*	*Moisture per cent*
Farm sample	0.4	0.3	0.4	83
Rothamsted	0.54	0.32	0.67	66
French figure	0.58	0.30	0.50	79
Rothamsted average	0.64	0.23	0.32	76
Fresh liquid	0.044	0.05	0.35	98
Old liquid	0.026	0.014	0.22	99
Stable manure	0.76	0.56	0.65	62
From bullocks	0.62	0.26	0.72	..
From cows	0.43	0.19	0.44	..

These figures probably indicate rather higher values than those generally attained in farming practice, for most of the samples were taken at research stations where the making of the manure had probably been carried out with exceptional care. Dr. Sanders in his *British Crop Husbandry,* concludes that F.Y.M. can contain from 0.4 to 0.8 per cent nitrogen, from 0.2 to 0.5 per cent phosphoric acid, and from 0.4 to 0.8 per cent potash. Our list of analyses suggests that these upper limits are very high and

exceptional. A *good* rough average would be: 0.6 per cent nitrogen, 0.3 per cent phosphoric acid, 0.5 per cent potash.

One of the most striking things about this figure is the relative lowness of the phosphoric acid figure. From the early days of the NPK ideas, poverty in phosphoric acid was regarded as a distinct disability of farmyard manure; for crops in many cases need as much phosphorus as nitrogen or potassium, and then F.Y.M. is an unbalanced NPK supplier.

We must also have some idea of the amount of nitrogen which is soluble, and the amount, too, of phosphoric acid that is soluble; though the potash can be assumed to be generally active since it does not form insoluble organic compounds. Again this division into soluble and insoluble will vary, for much depends upon the amount of urine conserved. It is mainly there that the soluble nutrients are found. It is safe to say that at least two-thirds of the nitrogen in F.Y.M. will be of the insoluble, complex organic kind. The animal excretes something like a 50 to 50 balance, but the inevitable losses in the soluble form must bring down this ratio. Indeed, two-thirds is an estimate that is in general over-fair to F.Y.M. It should not be forgotten that some of the initially active and soluble nitrogen is turned into complex nitrogen by the bacteria using it as food to decompose the litter, and this is a further unavoidable decrease in the quantity of active nitrogen finally available.

For phosphoric acid, as much as five-sixths of the total amount present may exist in an inactive form. This will be in an inactive organic form rather than in the inactive inorganic form, and there is reason to believe that this complex organic form is more readily brought into active nutrient form than insoluble calcium tri-phosphate. It is not of much value to discuss these matters qualitatively. There is one method and one only by which to decide to what extent F.Y.M. is effective as an NPK supplier, the method of direct field tests.

Here are some figures from Rothamsted, quoted by Sir Daniel Hall. The test compared the nitrogen effects of F.Y.M. and ni-

trate of soda. In both cases, the phosphorus and potassium supplies were supplemented by additions so that these variables were turned into constants. The crop was mangolds. Yields are expressed as roots only.

	F.Y.M. (= *200 lb. N*)	*Nitrate of Soda* (= *86 lb. N*)	*F.Y.M.* (= *200 lb. N*) *plus Nitrate of Soda* (= *86 lb. N*)
1900	28 tons	33.1 tons	41.8 tons
1907	26.5 tons	32.8 tons	42.1 tons

The yield from 86 pounds of chemical and active nitrogen is greater than the yield from 200 pounds supplied totally in F.Y.M. And, very significantly, a powerful crop increase is recorded when 86 pounds of active nitrogen is added to the 200 pounds in the natural manure. Not only does this test show the comparative slowness of manure nitrogen; it shows, as Hall pointed out, that—where high yields are aimed at—it is better to supplement the dung supply with fertilizer nitrogen than to increase the amount of dung. The argument may be made that it is unfair to judge F.Y.M. upon one-year effects, that its benefits may be slow but lasting. Again, the test covers this point for the soil had been regularly manured for at least thirty previous years, and so the yield in either of the years quoted above includes full residual effects from previous applications.

This probably clears up a point that may have puzzled some readers in chapter five when figures showing the nitrogen losses from manured plots and NPK fertilizer treated plots were discussed. Why was 200 pounds of nitrogen from manure compared with a much lower nitrogen supply from fertilizers? Because these ratios had been chosen to obtain a cropping level similarity for the two supplies of nitrogen.

A test with potatoes was carried out in 1924 by the County Council of Lancaster, and the figures clearly show the value of reinforcing the NPK content of F.Y.M. with more active forms of these nutrients.

Treatment	*Crop Weight*
10 tons F.Y.M.	9 tons
10 tons F.Y.M. plus 5 hundredweights NPK	10.7 tons
10 tons F.Y.M. plus 10 hundredweights NPK	12.8 tons
20 tons F.Y.M.	10.7 tons
20 tons F.Y.M. plus 5 hundredweights NPK	13.1 tons

The increases produced by the NPK additions are remarkable when compared with that of doubling the 10 tons application of F.Y.M. The NPK fertilizer used had an analysis of 5 per cent nitrogen, 15 per cent soluble phosphoric acid, and 12 per cent potash. Assuming the average figure for F.Y.M. we can compare the NPK values of the two applications as follows:

	Total Nitrogen pounds	*Total Phosphoric Acid pounds*	*Total Potash pounds*
NPK, 5 hundredweights	28	84	67
F.Y.M., 10 tons	134	67	112

Yet, whether 5 hundredweights of fertilizer or 10 tons of manure was added to the basic dressing of the first 10 tons of manure, the crop increase was the same—1.7 tons.

Here is another direct comparison test, this time from a test that was continued for more than 40 years, from 1894–1935 at the Ohio Agricultural Experimental Station, United States. The changes in the organic matter content were measured in this test as well as the crop increases. These are expressed on the basis that the initial organic matter content was 100 crop weights in bushels.

The NPK supplies were as follows (assuming an average value for the manure):

	Total Nitrogen pounds	*Total Phosphoric Acid pounds*	*Total Potash pounds*
Manure, 5 tons (metric)	60	30	50
NPK, 500 pounds	50	25	50

Yields and Organic Matter changes:

	Average Yield 1894–8	*Average Yield 1931–5*	*Organic Matter in 1935*
Continuous Corn:			
No manure or fertilizer	26.3	6.5	37
F.Y.M., 5 tons	43.1	30.0	53
NPK, 500 pounds	44.5	28.9	35
Continuous Oats:			
No manure or fertilizer	28.2	14.2	64
F.Y.M., 5 tons	34.8	34.6	97
NPK, 500 pounds	48.8	38.8	91

For both crops, the lesser quantities of chemical nutrients have given better results than those of the manure—*for the first 5 years.* The pull upon organic matter has been much heavier for corn than for oats; and, as the tests proceeded, the yield-differences reduced, and with corn the better result was eventually from the manure. Nevertheless, we can still draw the same conclusion that chemical nutrients are more effective, unit for unit, than the same amounts supplied in manure, with the qualification that this greater effectiveness depends upon an adequate and simultaneous maintenance of the organic matter level.

Many more tests could be quoted to demonstrate this same point, but these seem sufficient. They explain why the introduction of chemical fertilizers raise crop yields—and they explain too that this benefit must not be taken for granted, that it can in general be kept up only if close attention is paid to the supply of humus as well as NPK nutrients. One point seems clearly established: we cannot expect that more than 50 per cent of the total nitrogen supplied as F.Y.M. will be immediately effective in providing nutrients for crops; whereas most of the nitrogen in soluble chemical fertilizers is available for crop nutrition.

However, we cannot limit our considerations to F.Y.M. alone. There are the various compost manures to be considered too.

The analyses of composts depend so much upon the ingredients of the compost that it is clearly difficult to arrive at reliable *average* figures. In addition to this natural difficulty, those who

have enthused about compost have, on the whole, tended to ignore the NPK aspect. Their point of view seems to be that NPK considerations are out of date, that compost makes these calculations unnecessary—therefore, why bother to give *chemical* figures that are meaningless. There is, therefore, a general absence of data for the NPK contents of compost manures.

Here are three figures for ordinarily made compost, and one for *controlled tip* compost.

Materials Used	*Method*	*Nitrogen per cent*	*Phosphoric Acid per cent*	*Potash per cent*
Hop waste, animal manure, household waste	Indore	0.96	2.45	0.62
Straw plus 150 pounds of chemical nutrients per ton of straw	Adco	2.19	1.20	0.58
Rothamsted Compost (1937)	Adco	0.44	0.25	0.18
Refuse tip, Manchester	——	0.8	0.5	0.3

The worst of these figures shows a fair comparison with F.Y.M. The others are better. We have to reflect that many composts will be made from low NPK materials, and that the added nutrients could often have been used directly as fertilizers. The compost enthusiasts will disagree with this approach in terms of NPK value, but it must be insisted upon if the theory that natural manures can balance the whole humus *and* nutrient budget is put forward. There is no guarantee that a satisfaction of humus needs will *ipso facto* mean a satisfaction of NPK needs. If so, why did the introduction of fertilizers raise cropping levels? If so, why does an amount of NPK as farmyard manure so often produce lower crops than lesser amounts of NPK in fertilizer form? These questions cannot be left outside the present argument.

What average NPK analysis can be fairly taken for compost manures? In view of the fact that the production of much greater quantities of compost manures must mean more vegetable matter in proportion to a somewhat constant supply of animal manure,

we should anticipate somewhat lower figures than for F.Y.M. but better balanced figures, the phosphoric acid (from vegetable matter) being relatively higher. It would be very fair to composts to assume an average round about: 0.5 per cent nitrogen, 0.4 per cent phosphoric acid, and 0.5 per cent potash. It must be realized that this represents the compost-derived nutrients; composts reinforced during processing with substantial amounts of fertilizer materials, whether chemical or organic, are reinforced with fertilizers, and the higher analyses thus obtained are not exactly admissible figures so far as this budgeting is concerned.

There is, however, some evidence that compost manures are more efficient than F.Y.M. The evidence is both insufficient and short-term so far as properly designed tests are concerned. However, there is also a fair amount of general observation evidence, especially for the Indore mixed-animal-and-vegetable-matter composts. Here is one test figure from the Haughley Research Trust Farm, quoted by Lady Eve Balfour in her book, *The Living Soil.* Crop: potatoes. Period: one season.

Variety	*Yield from 6 Loads of Compost*	*Yield from 12 Loads of Farmyard Manure*
King Edward	7.5 tons	7.95 tons
Majestic	12.85 tons	13.15 tons

This apparent superiority of compost over F.Y.M. was again demonstrated in the second season of this test. Also other users of compost manures state that they find compost superior to equivalent amounts of F.Y.M.

My chemical skepticism, however, makes me want to ask one or two questions about these tests. I would like to have known the moisture contents of the compost and the F.Y.M. For, if the F.Y.M. had been rather higher in water content, the dry matter value of 12 loads of F.Y.M. might very well have been no more than that of 6 loads of the compost. I should certainly like to know the relative NPK values of both manures in the test.

Further, the soil under test may have contained enough humus initially in which case any greater amount of organic matter in

the 12 loads of F.Y.M. would have been merely the provision of excess. This could have been settled by comparing the results of 6 loads of compost and 6 loads of F.Y.M. It will, of course, tend to be settled by the results from the long-term continuation of the comparison. If the advantage of the compost dressing over the F.Y.M. dressing is maintained, then the advantage is more firmly established.

At the present stage it seems dangerous to say more than that it is strongly indicated that compost manures are more efficient than F.Y.M.

The NPK value of sewage sludges depends very considerably upon the dryness or wetness of the sludge. Here are some figures that provide a rough idea of these values:

From Routine Analyses by the Midland Agricultural College

18 samples of wet sludge and 11 samples of dry sludge varied between these limits:

	Nitrogen per cent	*Phosphoric Acid per cent*	*Potash per cent*	*Organic Matter per cent*	*Moisture per cent*
Wet samples	0.3 to 1.3	0.4 to 1.0	0.5 to 1.2	13 to 46	50 to 84
Dry samples	1.4 to 2.0	0.7 to 5.0	0.7 to 1.1	29 to 44	8 to 10
Royal Commission on Sewage Disposal, 1906 (Average of four samples)	1.4	1.4	traces	33	21
Specialized Sewage Product (A sewage product prepared in excellent dry friable form)	1.6	1.8	traces	variable	10 to 15

These figures indicate that *dry* sewage sludge, apart from the potash content, will provide better nutrient value than average F.Y.M. or the average compost manure. However, we cannot ignore field-test results, and in general these have shown very poor results, results not at all comparable to those of farmyard manure. This is perhaps to be expected—the soluble nutrients have almost all been lost in the unreclaimed liquid fraction, the toilet and the non-separation of other water wastes from sewerage

have swallowed up the active fertilizer value of sewage products. To say that one ton of sewage material would be equivalent to one ton of F.Y.M. would be an exaggeration without any justification. Nevertheless, it is difficult to give any rough quantitative figure for equivalence owing to the small amount of research work so far carried out on this problem.

Lastly, peat. The formation of peat—in long years of waterlogged bacterial decay—has washed out all the soluble nutrients. Peat must be regarded as a nitrogenous form of organic matter, with its nitrogen in complex inactive form. Figures vary from 0.5 to 2.0 per cent of nitrogen, and with phosphorus and potassium at trace figures such as 0.1 per cent or 0.2 per cent. The organic matter is high, a batch of American figures ranging from 70 to 94 per cent.

Having thus considered the main sources of natural manures from an NPK angle, we can make a fair estimate of an average analysis for all sources. This must be a very rough and uncertain step in the argument, but it will be a *fair* step if the estimate is on the generous side; fair, because the higher we assess the average value of the natural manure the less likely are we to establish a case for the additional necessity of chemical fertilizers. As a generous working assumption, then, I propose to give *all* types of natural manures (other than organic fertilizers) the average NPK content of:

Nitrogen, 0.6 per cent; phosphoric acid, 1.0 per cent; potash, 0.5 per cent.

This is definitely a higher figure than is suggested by the preceding data. The 1.0 per cent of phosphoric acid is given in recognition of the fact that, with increased composting activity, much more phosphatic waste will be reclaimed from vegetable matter which is not at present returned to the soil.

The next step is cumbersome. We must estimate the NPK needs of total agricultural cropping, and this is the kind of thing that again cannot be precise, that can give us only an intelligent indication. We shall have to tie up the calculations with some of the estimated figures in chapters four and five; for the soil's

NPK needs are not only replacements for the nutrients taken by crops, but also for nutrients lost by other causes.

It is fairly certain chemical knowledge that average crops of various kinds will take certain specific amounts of NPK from the soil. Here are a few examples:

		Nutrients in Pounds per Acre		
			Phosphoric	
Crop	*Average Yield*	*Nitrogen*	*Acid*	*Potash*
Wheat	30 bushels, grain	50	21	29
Potatoes	6 tons, tubers	46	21	76
Clover hay	2 tons	98*	25	83
Sugar beet	10 tons roots and 7 tons tops	90	35	169

* Beans being leguminous this nitrogen largely comes from the air not from the soil. In the following calculations legume crops are assumed to take no nitrogen from the soil, though in fact they probably do take some.

Nutrient loss by crop removal by 0.5 million acres of potatoes:

Nitrogen	11,000 tons
Phosphoric acid	5,000 tons
Potash	19,000 tons

I should explain that in all these calculations I have adopted the policy of rounding figures off, and this seems justified for we cannot possibly reach an accuracy of accountancy order—all we are going to get at best is a guide to the scope of the problem.

Including the permanent grass cropped for hay *the total losses* turn out as follows:

Nitrogen	200,000 tons
Phosphoric acid	120,000 tons
Potash	338,000 tons

On the whole I think these figures are well on the low side of the truth. For considerable help has been assumed from leguminous plants in the grass crops, and in the calculation for 0.7 million acres of *other crops* very low nutrient average losses have been taken although some of this cropping will actually have

been very intensive and therefore heavily nutrient consuming. However, from the present point of view, it is fairer to underestimate, for this makes it harder to establish a case for fertilizers. Also, losses of nutrients in permanent grass are ignored in this survey, though there certainly must be some losses in phosphoric acid and potash, even if natural means of nitrogen intake balance the removal of this nutrient by grazing cattle. This is a restricted survey—involving arable land and rotation grass and only permanent grass that is cropped for hay.

It is impossible to decide how much of the removal of nutrients is balanced by the flow of active nitrogen, phosphorus, and potassium from the soil's own inherent stock. Different crops have different persuasive capacities so far as extracting these nutrients is concerned; different soils vary in the tightness with which they hold their stock. Surely the sanest assumption is still the original assumption of Liebig's—that we must put back into the soil, or at any rate try to, as much as we take away. We must balance the budget so that the stock remains the same. In practice, fertilizer recommendations (by impartial advisers, not by commercial firms selling fertilizers) are frequently greater than the total of nutrients removed per acre by the average crop. Thus, a standard recommendation for potatoes in nitrogen–phosphorus–potassium order is 70–70–140 pounds per acre; compared with an average crop removal per acre of 46–21–76. Such a recommendation, of course, aims at a much better than average crop. Phosphoric acid applications are usually twice the crop removal figure because it is estimated that about half of any application is permanently held by the soil in the insoluble form to which soluble forms of this nutrient rapidly revert. How can we possibly assume that fertility maintenance will be achieved if we aim at anything less than fully balancing our running losses? This is indeed an underestimate of the problem, for many soils will be of low fertility and for these more substantial additions of nutrients will be needed to raise the fertility to average level. However, so long as the rough justice methods of this survey are always weighted in favor of the case against fertilizers, we shall

arrive at evidence that is sound because it points not to a doubtful maximum of nutrient need but to a certain minimum.

Next, we must add to our figure for nitrogen loss by cropping the serious nitrogen loss by leaching. Going back to the data in chapter five for the leaching loss we have research figures measuring this loss for arable land as follows:

With heavy F.Y.M.: loss of 143 pounds per acre per year.
With normal fertilizer N: loss of 51 pounds per acre per year.
With no treatment: no loss but very poor crops.
Rich soil: 68 pounds loss per acre per year.

Now these are the losses of *known* additions of nitrogen and of nitrogen inherently present in the soil. However, to them should be added the unknown contribution of the azotobacter which has not been added to the credit side of the budget. Whatever the azotobacter won from the air was lost in addition to these calculated amounts. Taking a conservative view of this loss let it be assumed that the average loss per arable acre was 40 pounds of nitrogen per year *plus the azotobacter gain.* This gives us for 7 million acres a loss of 125,000 tons. The leaching loss does not apply to rotation grass, nor to grassland of any kind.

For the phosphoric acid figure we must double the actual removal loss in order to arrive at the figure for replacement—owing to the point already mentioned that the soil permanently locks up about half of all phosphoric acid additions. The leaching loss of phosphoric acid is generally regarded as about 10 per cent, but we can take the 10 per cent on the undoubled figure, because leaching will hardly take away that part which the soil locks up so tightly that it is permanently inert. This brings the phosphoric acid figure to 252,000 tons, which we might as well round off at 250,000 tons.

The potash figure also can be said to carry about a 10 per cent leaching loss; so this figure becomes 370,000 tons. Many heavy soils are able to supply potash fairly readily out of store, and, though in general we decided that we should stick to a policy

of putting back whatever left the soil, in some soils potash availability is so marked that we should make some exception. It is purely guesswork, but let the figure of 370,000 tons be reduced to 300,000 tons.

We have, then, these deficits:

Nitrogen	325,000 tons
Phosphoric acid	250,000 tons
Potash	300,000 tons

The next question is, what gains are there to bring about a balance? There are no natural gains from the air for phosphoric acid or potash; we must look entirely in the direction of manures and fertilizers. For nitrogen, however, we have the three methods of deriving air nitrogen: leguminous bacteria, azotobacter, and rain. We have already entirely allowed for the leguminous gain since we assessed no nitrogen removal where such crops were grown. We have covered the azotobacter gain on 7 million acres, but not where there is no leaching loss. Taking a very good figure for azotobacter contributions: 25 pounds per year per acre —and assuming this for the remaining 2.25 million acres and for the 4.8 million acres of permanent grassland, we are being over generous to the azotobacter in assessing a total gain by their work of 80,000 tons per year. Note that this gain is going into grassland, and only if this land is eventually turned into arable land will this gain become generally available.

Then there is the rainfall gain, which has been measured to average 4 pounds per acre per year. For the whole acreage (9.25 million acres) in this survey, this will give a gain of 25,000 tons.

So we have for nitrogen:

Loss = 325,000 tons. Gains = 105,000 tons. Deficit now = 220,000 tons.

How much nitrogen was supplied by F.Y.M. in 1934? There were 0.88 million horses in agriculture, 6.7 million cattle, and 3.3 million pigs. Sheep we should disregard for most of their manure will have gone on to the permanent grassland not cropped for hay, which is not in the survey. Any sheep manure

which found its way into our budget will help to compensate for *some* of the F.Y.M. from horses, cattle, or pigs which went outside it. A standard Ministry of Agriculture figure (based upon research station data) gives a production of 150 to 180 tons of F.Y.M. per year per 50 head of mixed stock. However, we cannot assume maximum efficiency, nor can we assume that *all* the F.Y.M. that went outside the arable and hay-cropped land was compensated by sheep manure intake. For these two reasons, the maximum estimate of F.Y.M. ought to be about 25 million tons. We can exercise our generous instincts toward these gains by giving the average 0.6 per cent nitrogen content to all this manure, though in actual fact it is very certain that much of this manure will have heavy nitrogen losses. This gives an estimated gain from F.Y.M. of 150,000 tons of total nitrogen. In addition, this nitrogen is not all active nitrogen, and the loss we are trying to balance is of the active, soluble kind. In the Rothamsted test comparing F.Y.M. with fertilizer-supplied nutrients, similar sized crops were obtained only when 200 pounds of nitrogen as manure were applied against 86 pounds of nitrogen in soluble fertilizer form. Therefore, we must cut the F.Y.M. figure at least in half to express it *in terms of effectiveness*. The rest will either add itself to the soil's complex store, or be leached out when its conversion into active form is delayed until after crop-harvesting. Those who argue that everything that is *natural* is efficient, and who regard F.Y.M. as *natural,* will certainly not like this step in our budgeting. The answer surely is this: the Rothamsted measurements covering 70 years showed that over two units of F.Y.M. nitrogen were needed to give the same effectiveness as 1 unit of chemical nitrogen. The leaching loss, where this was determined, was 143 pounds per year against only 51 for the more economical fertilizer supply. If we are going to take the nitrogen supply from F.Y.M. at its full and total value, we must take a leaching loss not only of 40 pounds per year per acre, but of something like 100 pounds or even more. So, in halving the F.Y.M. contribution, we are still more than fair. Our nitrogen gain from F.Y.M., then, is 75,000 tons.

The figures for the other nutrients in this quantity of F.Y.M. are: phosphoric acid, 75,000 tons; potash, 125,000 tons.

In 1934 the consumption of chemical nitrogen from all sources was about 50,000 tons and we can reckon that all of this was effective, soluble nitrogen. Again with rough justice, we can assume that all of this was applied to the arable land or to the hay-cropped grassland. We have, then, as contribution from F.Y.M. and fertilizers of chemical kind *a total tonnage of nitrogen in effective form of 125,000 tons;* deduction shows a deficit still remaining of 95,000 tons. This should not be regarded as a staggering figure. It is indeed a very low figure compared with estimates of the loss per year in America and in many areas of Southern Europe.

However, the point we started out to consider is not the actual deficit as things are or have been, but this claim that fertility can be perfectly maintained by natural manures alone. We must go back to the net deficit of 220,000 tons of nitrogen and look at the problem afresh. There would still be the F.Y.M. contribution of 75,000 tons of nitrogen, and it would be fair to bring this up a little in view of the possibility that less nitrogen might be lost in a composting process than in the usual farm methods of handling. The compost school claims this point, so we will concede it to the extent of a 10 per cent gain, which makes the nitrogen value 82,500 tons of effective nutrient. This makes the deficit 137,500 tons. Disregarding the fact that much of this nitrogen may have to come from compost-aiding fertilizers (and we should also remember that some of the nitrogen in composts, whose analyses led to this average figure, had actually come from the nitrogen of the incorporated F.Y.M., which makes a rough calculation of this kind exceedingly complex), this means that we shall need at least 23,000,000 tons of compost manure even if all the nitrogen therein was effective; or about twice this, 46,000,000 tons, if the nitrogen was not more effective than that of F.Y.M. This seems a very large quantity.

To provide 46,000,000 tons very much more than this weight of raw materials would have to be handled initially. At inter-

mediate stages a much heavier tonnage would have to be turned for aeration, etc. Where will all these wastes come from? What labor will be needed for this task? And relatively it is not an absurdly astronomical high figure. Considering 9.25 million arable acres plus 4.8 million acres of hay-cropped grassland, the total acreage in our survey, we are providing with the F.Y.M. and compost manures together only a little more than 5 tons per acre per year.

On the supply side some figures given in the *R.A.S.E.** *Journal* for 1942 are informative. In the 15 months ending February 28, 1942, the *wartime* drive for household wastes had yielded for agricultural purposes:

	Tons
Manures and pulverized residues	235,634
Organic fertilizers	56,577
Bones and bone meal	18,655
Waste fish and fish meal	10,322
Meat meal and dried blood	9,643
Screened dust	86,239
Kitchen waste	270,908

This totals some 688,000 tons at a round figure, and some of the items of considerable tonnage can hardly have had a very high NPK value.

Of course, the sewage sludge source can help a great deal, but it is no widow's cruse. Forty-five million people are not likely to produce more sewage sludge than the F.Y.M. produced by 11 million cattle, even if reclamation efficiency is trebled or quadrupled. The controlled tip, the beaches that form reservoirs for seaweed, sawdust from timber-handling factories—these sources will help.

On the labor side, it must be said that the problem on such a scale as this would have to be a mechanical one, and therefore any calculations based upon more laborious hand-composting methods would soon be obsolete. E. B. Balfour has stated that two men can produce 1,000 tons of finished compost per year on the

* Royal Agricultural Society of England.

farm. On this basis, we should need some 90,000 men—against the peacetime agricultural manpower figure of about 650,000. This is for manufacture *on the farm,* and does not allow for the labor involved in collecting and transporting wastes to the farm. Some wastes now dumped have got to be handled anyway, but wastes such as seaweed would in many cases involve a new call upon labor and transport.

Let no one think that this is an attempt to discourage composting as a standard farming practice. On the contrary, there is a powerful argument for bold planning in this direction, an argument based upon the humus needs of the soil. The argument I seek to establish by this quantitative analysis is that it is impractical to look to composting for the soil's total NPK needs as well. It seems far sounder to look to manures of all kinds for humus and to fertilizers of all kinds for additional nutrients.

Many of the composting successes have been recorded in the colonies where labor is very cheap and where, in the words of one of these practitioners: "wastes can be had for the asking." Neither of these advantages is likely to apply to an industrialized country, where the manufacture of fertilizer forms of NPK can be carried on with all the economic benefits of production efficiency.

There is striking confirmation that my figure of about 5 tons of compost per acre per year is correct from Balfour's findings. She claims to have recorded consistently good cropping levels for 15 consecutive seasons on annual 5-ton dressings. Thus, by my chemical-minded approach via NPK reasoning, I have reached almost perfect agreement with a practical finding from the compost school; though they, on their side, would never admit the validity of the chemical outlook.

Unless it is devastatingly clearly proven that fertilizers are dangerous aids to soil fertility (which we shall consider in Part Two), it seems far more sensible to aim at a lower maximum for our manure needs, a maximum sufficient for humus maintenance, and to add the rest of our NPK needs for high-level cropping in the form of fertilizers. For, as we have seen so far, there is con-

siderable powerful evidence for the view that we can rely upon fertilizers for cropping assistance.

However, this budget is concerned with 1934 cropping, and to assume such a level for the future is to assume a none too healthy agriculture with arable acreages declining and acreages of untended land increasing. To go to the other extreme, we can glance briefly at the balance sheets drawn up by Colonel G. P. Pollitt in his book, *Britain Can Feed Herself*. After allowing for a maximum F.Y.M. production in his plan, Pollitt finds he still needs 673,000 tons of nitrogen to balance his budget, and without considering leaching loss. This would involve some 200 million tons of compost manures (my estimation here, not Pollitt's, for he ignores compost possibilities). Pollitt brings the leaching loss allowance into his calculations at a later stage of his argument by assuming that half of all his added nitrogen is bound to be lost by leaching. Even granting a lesser leaching loss to composts (which is not indicated by F.Y.M. figures on this matter in any case) there would therefore be required from *300 to 350 million tons of compost!* Pollitt, going straight to fertilizers, plumps for 6.7 million tons of sulfate of ammonia or equivalent forms of chemical nitrogen.

In 1944 the Ministry of Agriculture gave publicly the following cropping figures for the British wartime level of food production: 3.2 million acres of wheat, 1.9 of barley, 1.4 million of potatoes, etc. The total acreage of arable land was given as 19.3 million acres with about 0.75 million acres *more* temporary grass than prewar. These figures can provide another estimate of nutrient loss and needs.

On this basis, the nitrogen removed by crops is round about 100,000 tons more than for the 1934 cropping, and the extra leaching loss through the greater arable acreage (other than grass leys, etc.) about 60,000 tons.

The extra need for compost, extra to the 1934 level of cropping, would be in the order of another 30 to 40 million tons; it is not possible to tie the figure down more closely for we cannot assess the various factors of balance in a future agricultural

effort, e.g., the balance between stock and arable farming which would decide the amount of F.Y.M. available, or the extent of alternation between arable and non-arable land. On the 5-ton per acre view, the figure would be 25 million extra tons of compost, assuming (I think wrongly) that no compost would be needed for grass leys. In all, then, for a post-war agriculture at approximately the 1944 level, we should need about 70 million tons of compost in addition to the 25 million tons of F.Y.M. assessed on a 1934 stock population basis. If the 25 million tons of F.Y.M. rose through more stock farming, then the compost figure could fall to the extent that the F.Y.M. came from cattle foods imported from other countries (and that might be only to a small extent). Could this amount possibly be found? Could we increase the agricultural labor force to handle the job?

In most of the cases, it will have been noticed that nitrogen only has been considered. The discussion has been aimed at one point only—the hypothesis that fertility can be maintained by natural manures alone without the use of fertilizers. A simple calculation will show that whenever a figure has been worked out for the amount of compost needed to supply the nitrogen requirement of a cropping level, then this quantity will also provide more than enough phosphorus and potassium to satisfy deficits of these nutrients. The supply of compost manures would, therefore, solve the nitrogen and phosphorus and potassium problems at the same time, whereas nitrogenous fertilizers have to be supplemented with phosphatic and potassic fertilizers.

It comes then to this. If you believe that the natural manures can solve the fertility problem alone (assuming, of course, a reasonable standard of fertility-maintaining husbandry, for neither manures nor fertilizers can compensate for bad farming), if you believe that the labor and raw materials for these very great tonnages of compost manures can be provided, then there is no solid case for fertilizers, except possibly to a comparatively small extent as compost aids. If you do believe this cannot be done, you must still admit that so far fertilizers have not been used sufficiently to balance the soil's budget particularly for nitrogen.

There is therefore an overwhelming case for as much composting as possible, for a greater production and use of fertilizers, and for more attention in farming to the old fertility-conserving practices. *There is room for all methods of balancing the debits;* and it seems unwise to adopt extremist views of any kind, to say that it must all be done with chemicals or by composting or by ley-farming practices and so on.

I doubt if we can afford to enjoy the advice of any dogmatic school, so serious is the problem. To say that in future all peoples of the world are going to be better fed is perhaps only a political statement—scientifically it is an expressed aim, and no more than that. If, however, it is to become an actuality, it will depend upon soil fertility, and it seems unwise to ignore this basic fact. Erosion has swept away millions of so-called *cheap food* producing acres, and in the battle to escape further erosion many more large areas are to some extent locked up. Enormous agricultural areas in Europe produce such poor crops that no English farmer could make any kind of living from them. There is more than straw-in-the-wind evidence to suggest that India is unable to produce enough food for large parts of her population; famine is at any rate a tangible kind of clue. Those who think it will all be quite easy, because the world used to burn crops in the depressed thirties, need not regard that wretched fact as evidence that there ever was more food produced than the world needed. Food was burnt for one reason only, to keep up the price of the food that was not burnt. While the food burnt, people starved. It is commonly assumed that the better postwar feeding of the world will depend upon adjustments of the economic systems of international food distribution. However, even before food can be bought and sold, even before this new generation of reformed characters can operate world markets, the soil must produce the food. Our pious hopes for a decently nutritioned world may well depend upon the intelligence with which we try to balance NPK and humus losses.

There is just one point that should be fitted into this picture. The use the world makes of fertilizers, assuming for the moment

that the case for fertilizers is established, depends upon two limiting factors: first, there is the farmers' economic ability to buy fertilizers, which in turn depends upon what money reward he obtains for the food he grows, second, the production capacity of the fertilizer industry, which, though it can expand if the demand exists, is fixed by practical limits at any one time. If the soil budget is to be better balanced, then farmers must have businesses that are stable enough for them to spend the money upon the necessary fertilizers; and, second, the producers of fertilizers must be able to count upon this stability lasting long enough for them to expend capital on setting up more factories. In the past, apart from abnormal wartime periods, fertilizer production and consumption has on the whole expanded at the rate of farmers' readiness to buy. This has certainly shown a steadily increasing expansion, but any real and determined effort to set about balancing soil budgets would imply a much greater rate of fertilizer industry expansion. At the present rate it would be many years before enough nitrogen would be fixed from the air to equalize the soil losses. Against this, however, must be set the fact that nitrogen is needed in war for bloodier budgets, and to meet this emergency demand the resources of states are mobilized to construct nitrogen-fixing plants and nitrogen-producing factories with the result that, after each war, the world finds itself with a greatly increased capacity to turn atmospheric nitrogen into chemical nitrogen. This is, indeed, the modern version of the sword and the plow transformation. Of course, those who believe that chemicals are not beneficial to fertility look upon this in a very different way, and they accuse whoever owns these factories after a war of forcing chemicals into the land in order to continue to make profits. This school of thought invariably describes the nitrogenous legacy of war as a curse, while the other school who believes in fertilizers regards it as an indirect benefit from war. The argument has even been put forward that the states encourage excessive nitrogen fertilizer production and consumption so that, if war comes, there is an adequate industry to be switched over to nitrogenous explosive manufacture.

CHAPTER VIII

LIME AND FERTILITY

THIS BOOK so far has predominantly treated the additions of NPK chemicals or humus, the third need of the soil—lime—has been much understressed. Quite apart from the fact that calcium is a most important plant food, the correction of soil acidity is vital both to the availability of fertilizers and to the biological functions of the soil's bacteria.

In the first chapter we talked about assessing the soil acidity by the measurement of the pH. This pH business is a big obstacle to all scientists who try to write for the ordinary reader. It is a mathematical as well as a chemical symbol; most chemistry students have quite an interruption in their progress while they wait for its true meaning to dawn upon them. For the sake of completeness I am going to have a shot at explaining it in simple terms; but I would add that, if this attempt fails, the idea of the pH measurement can still be accepted simply as a number like the reading on a thermometer.

The ordinary method of finding out how acid a material is merely involves the addition of a measurable amount of alkali until the mixture is neutral to some indicator, i.e., is neither acid nor alkaline. The amount of acidity is equal to the amount of alkali required to bring this neutral state about. Actual acidities are not the same as total acidities. The addition of the alkali calls forth all the acidity of which the material is capable, the actual amount and the potential amount. Acidity is a state, a condition—it is not an amount of anything. The acidity of an acid is the extent to which the acid will split up into ions in solution, and particularly into hydrogen ions. All acids can be designated by a formula HA. Nothing can be acid without hydrogen in it. Now when HA is in contact with water (e.g., the soil solu-

tion in the soil) there is *some* ionization, which means that $HA \rightarrow H^+$ and A', where the hydrogen ions are positively charged and the A ions are negatively charged. So the acid is really some unsplit HA and some H^+ and some A'; but the *acidity* is due only to the amount of H^+ present. When we add alkali, these hydrogen ions are neutralized and more come out of the previously unsplit HA to replace them and these are then removed also until in the end all the hydrogen has been taken up by the alkali; thus the alkali addition measures the whole acidity, actual and potential, of the HA. Strong acids are those in which nearly all the hydrogen splits off as hydrogen ions whenever water is present and whether alkalis are added or not; weak acids are those that remain mainly un-ionized as HA and are only to a small extent split into H^+ and A'.

Now the soil's acidity is caused by weak acids. And we are not really concerned with how acid the soil might be if strong alkalis were added to it, but only with the actual state of acidity in normal circumstances. It is that which will upset the bacteria or affect the availability of fertilizers and so on. In short, what we want to know, and to change if necessary, is the concentration of H^+ in the soil solution. This is where the pH business comes in. The pH measures the hydrogen ion value; it is not itself the value, it is a logarithmic number related to it; but the mathematical side must be taken for granted.

It is rather like acidity of character in human beings. One acid person might be unpleasant only in certain circumstances or when certain people are present who bring out the worst; another person might be acid in all conversations with everybody. On a pH kind of value the latter would usually be far more acid than the former.

Whether this has cleared up the pH business or not, the following facts about pH values must be accepted:

If the pH is more than 7.0, the condition is alkaline.

If the pH is exactly 7.0, the condition is neutral.

If the pH is less than 7.0, the condition is acid.

Thus, a pH of 5 indicates more acidity than one of 6; and a

pH of 9 would indicate more alkalinity than one of 8. As the acidity increases, so the pH figure gets smaller.

An American view about soil pH values is as follows:

Extremely acid	below 4.5
Very strongly acid	4.5 to 5.0
Strongly acid	5.1 to 5.5
Medium acid	5.6 to 6.0
Slightly acid	6.1 to 6.5
Neutral	6.6 to 7.3
Mildly alkaline	7.4 to 8.0
Strongly alkaline	8.1 to 9.0
Very strongly alkaline	9.1 and over

For most cropping, the pH of the soil is best at a value round about 6.0 to 6.5, that is, just on the slightly acid side. At such a pH, bacteria of the helpful kind will enjoy good conditions; the various soil nutrients will be kept in an optimum state of availability; various fungi that cause diseases such as finger-and-toe will find unfavorable conditions, and the soil will tend to granulate to the right particle size. If the pH drops too much below 6.5 or 6.0, then phosphoric acid ceases to be so available. Toxic elements like aluminum or excessive iron become more available. If the pH goes up, that is toward alkalinity, then certain trace nutrients will become entirely unavailable, e.g., manganese, iron, copper, zinc, boron. And such a condition would be very hard to correct for there is no possibility of showering an acid upon soil to correct alkalinity, however easy it may be to correct acidity by adding the alkaline lime. There is, therefore, a lurking and serious danger in the overuse of lime.

What is known as the lime requirement of a soil is the amount of lime needed to shift the pH to the range of 6.0 to 6.5. No farmer should worry about this; he should hand the worry over to his local advisory chemist, who will sample the soil, measure the pH, and report upon the lime requirement. The farmer should worry only about deciding *when* he needs the attention of his regional scientists. Experience will tell him when lime is needed. Certain weeds thrive upon lime-needing soil, e.g., sorrel

and spurrey. Legume crops fail when lime is overdue. In any case, owing to the leaching of lime and its removal by cropping, most soils need liming once every 4 years.

The main source of lime is the chalk or limestone which is not actually lime—it is calcium carbonate, $CaCO_3$. It becomes lime only after heating, when the gas, carbon dioxide, is driven out and lime, calcium oxide, is left. $CaCO_3 \rightarrow CaO + CO_2$. That is what happens in a lime kiln.

Now this lime (burnt lime or quicklime or lump lime, or ground burnt lime) is caustic. It is urgently desirous of combining with water and a good deal of heat is given off when this union takes place; if it takes place on the skin or in the eye it is a very burning business. Once the job is done, once it has taken up the water it needs, the product, slaked or hydrated lime, is non-caustic and quite pleasant to handle, though it still is not the kind of stuff to get in the eyes or mouth.

Lime kilns sell the caustic lime as lump lime, quicklime, burnt lime, or as ground burnt lime. If they do more work for the consumer and slake the lime and then grind the product, then they sell the lime as hydrated or slaked lime.

Farmers often buy the burnt lime loose in lumpy form and leave it in a heap to slake itself; the heat generated drives off excess moisture, and a useful powdery slaked lime is left.

The original chalk or limestone can function as lime in the soil even though it is not really lime. The carbon dioxide will be driven out by other acids just as readily as by heat. So the farmer, if he likes, can buy the chalk or limestone without paying for all this extra processing, except that it must be finely ground because lumps of chalk or limestone will be of very little use to the soil.

The actual antiacidity value of all these products depends upon their contents of lime, the alkali that neutralizes the acid. These are average figures:

Forms of burnt lime contain about 95 per cent CaO.
Forms of slaked lime contain about 70 per cent CaO.
Chalk or limestone contains about 50 per cent CaO.

In terms of effectiveness, it is generally reckoned that:

Ground burnt lime 20 hundredweights = slaked lime 25 hundredweights = limestone 35 hundredweights.

It might seem at first sight that the only sensible practice is to buy the burnt lime in one form or another; it may cost more per ton but this is offset by lower cartage costs and handling costs. Hydrated lime might also seem a runner for first place but it should be pointed out that its production costs are highest of all, so that generally it is more economic for smaller-scale use in gardens, etc.

However, there is a powerful argument for the use of ground chalk or limestone. Burnt lime is always seeking moisture; even when ground to a fine particle size it tends to cake as moisture is sucked in from humid air, and it can do this to such an extent that the bags burst. When lump lime is naturally slaked, the powdered hydrated lime formed is often caked badly, with caustic lime embedded in the center of sticky particles. There is, therefore, little reliability about *the fineness of division* of any form of burnt lime. On the other hand, the quietly behaved limestone can be ground to a very definite and unchanging particle size. Particle size is a big factor in soil chemistry; the smaller the particles, the greater the total area of surface exposure. Ground limestone, then, has a better chance of speedy antiacid action in the soil. Against this, it must be said that lime is more soluble than limestone, twenty-three times more soluble. This is important for the lime material must dissolve in the soil solution before it can affect the hydrogen ions. So, in this respect, the argument for lime is supported. H. F. Taylor, in a paper recently published, balances these competing claims by pointing out that:

1. In general practice, the particle size of burnt lime is 100 times (or more) that of ground limestone.

2. This must be a more adverse factor than the favorable factor that lime is twenty-three times as soluble as limestone.

In short, ground limestone is likely to correct soil acidity at least as quickly as, if not more quickly, forms of burnt lime. (Though, of course, a greater quantity must be used, for these

factors do not affect the relative lime contents.) This is not unimportant for it is much easier merely to quarry limestone and grind it than to run kilns; and the production of burnt lime materials is never likely to equal the total lime needs of the soil. The lime handling industry is not one in which we should want to employ more men than are absolutely necessary; it is not the healthiest of occupations. The material which can be most economically produced and in the greatest lime value per man-hour is the material that should be used.

For further details of this comparison, two very thorough papers should be consulted: "Use of Carbonate of Lime in Agriculture," by Dr. R. Coles in *Agriculture,* April, 1943; and "Calcium Carbonate or Quicklime for Agriculture?" by H. F. Taylor in *Chemistry and Industry,* October 21, 1944.

There are also various kinds of waste limes from industry, e.g., beet factory wastes, residual limes from tanneries and tar-works, etc. Their value depends upon their analyses, and from the farmer's angle upon the distance of his farm from their place of by-production. They suffer from the handicap (usually) of being moist and tacky, and may therefore tend to be slow through offering a large particle size to the soil. In case of doubt about one of these products, expert advice should be sought from the regional adviser.

What kinds of dressings are commonly required? A dressing to correct bad acidity might be 2 tons per acre and upwards in terms of burnt lime; maintenance dressings each fourth year, however, might be as low as 10 hundredweights per acre. The usual application time is before or after plowing; big dressings are often split, half before and half after. For small dressings, there is again a lot to be said for the use of limestone; for it would be much easier to apply evenly 17 or 18 hundredweights of finely ground limestone than 10 hundredweights of perhaps rather sticky burnt lime. These figures have been given only as a guide, and it cannot be too strongly stressed that the farmer should take advantage of an expert's service. Liming is vital, but there is always that hidden risk of over-liming.

CHAPTER IX

LOOKING BACK

"The strength with which we hold a belief ought to bear some proportion to the amount of evidence upon which it is based. Often, however, we hold a belief much more strongly than the evidence known to us warrants; again, we sometimes refuse to entertain an opinion for which there is considerable evidence." The late PROFESSOR SUSAN STEBBING, *Thinking to Some Purpose.*

THE DETECTIVE novelist holds up his story from time to time to allow the virtuous characters to chew over the confusion of clues and do a little sorting out. This book seems to have reached a similar stage. However here the procedure must be adopted genuinely and not as the detective novelist adopts it, merely to put the reader on a clear track in the wrong direction.

Before doing this, it might be as well to reflect upon the aim and purpose of scientific research. At any rate in dealing with natural phenomena, it is the pursuit and disentanglement of truth, truth being the unconcealing of natural processes. There are two aspects of this pursuit. Science is doing its job very well if details of these processes are exposed, classified, estimated, or accurately measured. A statement that such a thing happens to such an extent and another to another extent, this—if clearly proved—is truth pursued and caught. The provision of a theory or working hypothesis into which all these details fit and which welds them together is a second step. To quote again from Professor Hogben's *Science for the Citizen:* "No sharp line can be drawn between organized scientific knowledge and mere rule-of-thumb methods of recognizing the characteristics of nature. The latter must always precede the former, and the former grows out of the latter."

It is a strength and not a weakness of science that the right is reserved to amend organized theories. Indeed, it is possible for there to be more than one such theory believed in at any one time by different schools of thought. This is especially likely where many of the factual details are still unrevealed by measured investigation and where the total problem is complex. However, in deciding whether both theories are tenable or only one of them, it is wise to measure them against the details that *are* known—for, if one theory fails to fit in with some of this concrete evidence, then it is hardly likely that it can eventually be proved correct by the later discovery of new facts. On the other hand, it should not be supposed that a theory which adequately fits all the known facts will not be amended by facts that emerge later, and so both theories (where there are two) might in truth be finally wrong.

Thus, we really have three ideas about additions to the soil to maintain fertility. Perhaps these three ideas are not equally seriously pressed. Certainly, there are less and less who would support the first. These are:

1. It is efficient to rely upon chemical fertilizers only.
2. It is efficient to rely upon natural manures only.
3. It is efficient to use chemical fertilizers as suppliers of nutrients, and to use at the same time natural manures for humus supply, the two types of addition being regarded as complementary.

In looking back at the evidence recorded by various pieces of research, how do these three ideas, these practical hypotheses, fit in with the known evidence?

There is still one warning note to be sounded. One of the general snags in scientific research is the frequent necessity to *de-naturalize* an effect in order to measure it. The very process of isolation for the purpose of measurement is often a removal of the ordained natural context. This is very true of measurements of the effects of specific fertilizers in the soil and of the effects of chemicals in culture tests. At what stage may a critic fairly argue that this kind of measurement is all of such an ab-

normal nature that it bears no relation to ordinary truth? I suggest that a critic can do so justly and soundly *only* when it is generally shown that the measurements and deductions obtained do actually conflict with ordinary experience. If, on the other hand, the evidence from these measurements logically helps in the interpretation of normal happenings, if there is not a steady conflict between facts of scientific isolation and facts of practical experience, then it cannot be argued that scientific method has so isolated processes that their assessment has been futile. In short, we should not accept test results obtained by abnormal treatments *unless* those results are also confirmed by other methods of inquiry.

With this attitude in mind, what case have we for the use of fertilizers?

1. *Evidence from plant-growth in soil-less cultures, water or sand.* Certain elements shown to be essential since growth cannot occur in their absence; other elements shown to be necessary for healthy growth if not vital to some kind of distorted growth. The elements concerned indicated in the first place by analysis of plants and plant ashes. *This is evidence of a very abnormal kind,* and it cannot be regarded as more than a series of indications. This kind of evidence, largely qualitative in nature, leads to the idea of fertilizers, but that is all. It does not by itself prove that they may be necessary or even useful in the soil.

2. *Evidence from plot-tests carried out with soil as the medium of growth.* Here there are thousands of tests, many carried out over long periods, but not carried out all at one place or by one group of scientists. In an accurately measured way they show that the additions of certain of these necessary elements to the soil cause crop increases. This evidence supports two deductions. First, that certain plant foods added to the soil in *chemical* forms can be utilized by plants; second, that the soil itself cannot supply as much of these foods from its own content as plants can use if given the opportunity. This is evidence of a fairly solid nature; it is not dismissible as a mere indication. The

regularity with which the same kinds of crop responses are given for the same quantitative additions of certain nutrients shows clearly that there is a reliable cause-and-effect relationship between growth and chemical additions to the soil.

3. *Evidence from general agriculture, i.e., not plot-tests.* The crop responses shown and accurately recorded in plot-tests have been frequently confirmed by results of fertilizer applications to large areas. Accurate measurement is not so easy in such cases and other important factors intervene, but, in general, the regular use of fertilizers has raised the average crop per acre by about that amount which plot-tests suggested. This kind of evidence assumes, of course, that fertilizers are properly used, for otherwise a large amount of contrary evidence could be brought forward from farming experience to show that fertilizers do not increase crops. Clearly, the overuse of fertilizers or the use of the wrong type for specific crops or soils must be ruled out as evidence in the same way that we should not regard it as evidence that steam engines are impracticable because somebody blows the boiler up by altering the safety valve. (This is very obvious but it is mentioned because many people are quite content to argue against fertilizers on evidence of their misuse.)

4. *Evidence from the soil's failure to support crops on its own nutrient content.* This is already fairly well proved by the fact that fertilizers increase crops as stated above. The *independent* chemical investigation of the soil is, however, important corroborative evidence for it shows that the soil's plant food content exists in both available and not readily available forms. Though there is no laboratory method that accurately imitates the extracting abilities of plants, the fact that various extracting solvents can distinguish between active and inactive kinds of phosphoric acid and potash gives a reasonable explanation of the paradox that soils may contain plenty of nutrients yet need help to support high-level cropping. The biochemical distinction between complex and simple nitrogen is even more obviously evidence of this same nature.

5. *Evidence from losses of nutrients from the soil.* The inde-

pendent measurement of losses from the soil by crop removal, by leaching, and by locking-up in inactive forms, is also strongly corroborative evidence, for it shows *why* the soil's rate of supply of active-type nutrients from its inactive store is generally tending to be insufficient. It shows, for example, even in the case of nitrogen, where there are natural ways and means of soil-content increase, that the rate of availability is likely to be outbalanced by the rates of removal and loss.

However, all these points still do not make a clear-cut case for the use of chemical forms of nutrients. For there are the natural manures—the animal manures and the compost manures. If supplies of these are sufficient to balance all losses of nutrients in necessary cropping, then there is no argument for fertilizers. Examination of this point has shown the following further points:

1. Farmyard manure is not as efficient per unit of nitrogen in giving *immediate* crop responses as are soluble-type nitrogenous fertilizers. To obtain similar cropping results, other things being equal, more than twice the amount of nitrogen in F.Y.M. is needed as compared with the amount in the form of chemicals. On the other hand, the organic content of the soil is better maintained by F.Y.M. than by fertilizers, when these are regularly applied.

2. To approach a nitrogen balance for any reasonable cropping standard, the need for natural manures would seem to be enormous, and it is very doubtful if this tonnage could be provided. The provisions of such a tonnage would, however, also balance losses of other nutrients and of organic matter at the same time. There seems no possibility of any approach to this tonnage at the present time. Also, when there were abundant supplies of F.Y.M. and when agriculture had to support a much smaller population, the cropping level was considerably lower than the level now obtained by the use of fertilizers and F.Y.M.

Nevertheless, the accumulated experience of many tests and investigations has shown that the NPK nutrients and the lime

status of soil are not the only variables concerned in fertility. The maintenance of the organic matter content—or, more accurately, of the humus content—is vital. Though there is some evidence seeming to point to the possibility that humus is not absolutely essential for plant growth, this evidence is not nearly as considerable as the evidence pointing the other way. On the whole, we must conclude that the organic matter provided only by crop residues is not sufficient to maintain the humus level. Which leads to a further point:

3. Additions of organic matter must be made to the soil for humus maintenance, and it would seem desirable to make up for the decline in animal manure supplies by much more intensive effort to reclaim organic wastes by the compost method. The amounts of compost and F.Y.M. required for humus maintenance will also provide useful quotas of NPK nutrients, but these will not be enough to support a modern cropping level without the complementary addition of fertilizers.

In the preceding chapters it can, I think, be reasonably claimed that evidence for all these points has been provided. It must be realized that the *whole* accumulation of evidence would require a number of volumes for its presentation—the evidence given has been a cross-section of its kind. Anybody who wishes to know just how much of this kind of testing is being carried out today should study the Imperial Bureau of Soil Science's bulletin of abstracts, *Soils and Fertilizers,* published several times a year; or the monthly abstracts of the Bureau of Chemical and Physiological Abstracts, Section B III, *Agriculture, Foods, etc.* I doubt whether any single person could possibly collate all the field tests with NPK materials which have now been reported in the various annals of scientific study.

Beyond the experimental scientific case there is perhaps the strongest piece of evidence of all, the steady expansion of the consumption of fertilizers in farming. This expansion has continued throughout the ups and downs of agriculture in the past century, and especially in this twentieth century. Fertilizers are

not ridiculously cheap; their price per ton is always an item of some significance. Farmers would not have gone on using fertilizers if they had not found that the scientific claims were confirmed in their own experiences.

I have been told that this last argument is not a strong argument at all, but a very weak one. This opinion was expressed by a member of the humus school with whom I differ, but for whom I have a very great respect. It was said that my argument was weak, because so many farmers were now turning away from chemical fertilizers. Now I might have been prepared to swallow this counterargument, if I were not closely connected myself with fertilizer production, if I were not in a position to know (and often be harassed by) the farmers' demand for fertilizers, which is greater now than it has been at any previously known time since 1840. The humus school, through their attack upon fertilizers, may well know of a number of farmers who have swung over to a prohumus policy. What they may not know is the enormous number of farmers now using fertilizers, farmers who before the war did not bother with or could not afford fertilizers, farmers who are using twice or three times the annual tonnage that they used to, farmers who are new men in the agricultural game altogether and who are applying fertilizers to land that perhaps has never known even lime for decades. I do not know the actual figures, they have yet to be published, but I know the problem of producing the fertilizers to cope with the demand, I know it to an extent of sweat and tears. I have certainly seen these expended in just one small fertilizer works—sweat mainly, and if not public tears then plenty of the nervous-strain-breakdown variety, the internal tears of the periodic despair that comes to keen men who are trying to do more than humans really can. When anybody tells me that farmers are going away from fertilizers, when they expect me to regard this as a significant statement of the slightest statistical value, I can only believe that they make the statement out of wishful thinking or out of most unfortunate misinformation. Of course, there are some farmers giving fertilizers up every season, just as there are some wives leaving their

husbands every week, but a few swallows don't make a summer, and it is what happens in most cases that really makes a generalization. The use of fertilizers is increasing, steadily increasing, and the rate of consumption will go on increasing unless there is a very marked reversal of current trend and past tendency.

The last paragraph has drawn upon personal experience. I distrust personal experience very much. I do not mean I disregard it for that would be ludicrous. However, I want it confirmed by other personal experiences, other being used here in the sense of other people's. So, if only to satisfy myself, I am going to reinforce my opinion about fertilizer consumption with some figures from the United States, figures quoted in the current *Applied Chemistry Reports,* 1943, published in 1944. In the section on soils and fertilizers, this is what Prof. A. G. Pollard remarks:

"D. C. Kieffer, of the American War Production Board, reports an anticipated production of 700,000 tons (as K_2O) of potash during 1944, of which 36,000 tons are to be allocated to Great Britain. The demand for superphosphate in the United States has reached unprecedented levels (6,600,000 tons for the 1943–4 season).

"Such figures and the urgency of the demand stand out in sharp conflict with Sir Albert Howard's policy of eliminating artificial fertilizers in favor of organic manures. . . ."

Data from the 1946 summer issue of the *Fertilizer Review* (National Fertilizer Association) show that in 1945 the United States used 13,202,000 tons of fertilizer, 9.5% more than in 1944. This 1945 figure represents an 80% increase on the average consumption in the period 1935–39. The report of the National Fertilizer Association goes on: "The present trend of fertilizer consumption continues upward, and indications are that, if materials are available, fertilizer use in 1946 may even exceed that of 1945. There is increased demand for fertilizer brought about by (a) the expanded food production program due to world food needs, (b) the increased use of fertilizer in newer fertilizer-using areas, (c) the increase of tonnage in areas that have long benefited by fertilizer usage, (d) the peak income of farmers, and (e) the favor-

able relationship of prices received by farmers for their products to the prices paid by them for fertilizer."

Some 1946 British figures have also become available. During April, 1946, the total consumption of superphosphate was 116,800 tons compared with a pre-war monthly average for this busy agricultural month of 39,400 tons; consumption of compound fertilizers in the same month was 231,100 tons compared with 65,700 tons in April, 1941. Again in the same month, 21,450 tons of nitrogen in fertilizer form was consumed against the average monthly consumption of 9,390 tons in 1940.

Therefore, with all due respect for any feeling the humus school may have that their policy is reducing support for fertilizers, I must assert as a fact and not as an opinion that fertilizer consumption is on the increase, and I suggest—now as an opinion and not as a fact—that this is evidence for the idea that fertilizers are effective in helping man to maintain and increase soil fertility.

I have no wish to prejudge the issue for any reader. Final opinion should be formed by readers themselves, and, preferably, if they have read other books as well as this one, provided they analyze all arguments and evidence carefully, and accept only logical deductions from planned and accurate investigation. Therefore I offer my own conclusion purely as a suggestion. It is the orthodox conclusion, but I do not think I have come to it just for that reason, for in other matters of opinion and debate I am generally on the unorthodox, non-bowler-hatted side. It is this: all this evidence we have considered and summed up in this chapter boils down to one basic principle—fertilizers and natural manures are complementary, and a high cropping level cannot be safely maintained without fertilizers for NPK and without manures for humus, with this subprinciple: that it *may* be possible to maintain fertility with fertilizers only, but such a policy depends upon a very expert indirect handling of the humus supply problem by leys, green manuring, catch-cropping, and similar practices. The operating word in this subprinciple is *may*—there is no certainty here, only possibility.

CHAPTER X

THE CASTE

"Liebig's name is associated with the first patent fertilizer and the first patent food ("Liebig's Extract of Meat"). Though neither of them were very successful, they point to the close connection between the physiology of nutrition and the rise of chemical manufacture." PROFESSOR HOGBEN, *Science for the Citizen.*

SO FAR in this discussion a fertilizer has been little more than some vague, unspecified material which, when added to a soil, will supply plant nutrients. And if this academic conception has been good enough for the time being, it certainly cannot be good enough any longer. The farmer buys superphosphate and nitrochalk and muriate of potash; the market gardener buys bone meal and hoof and horn; the small gardener buys Messrs. Blobb's nicely packeted all-things-to-all-plants compound fertilizer; and so on. It is time to be rather more specific.

There must be a clear distinction between fertilizers and manures. Manures are those materials which predominantly supply humus with NPK nutrients as a lesser component; while fertilizers are materials which mainly supply NPK nutrients, but in which humus may or may not be also present. Some materials fall in a no-man's-land between; however, in most cases, distinction can be clearly made. In scientific literature on this subject the distinction is fairly rigidly adhered to. In commercial literature there is still a selling-angle habit to describe as manures materials that are most definitely fertilizers; it is a nervous hangover from the days when buyers scoffed at chemicals and preferred to stick exclusively to farmyard manure.

What kinds of materials make fertilizers? Clearly, a fertilizer must be purchasable at an economic price. If the likely crop

increase is worth X, the amount of fertilizer must cost sufficiently less than X to cover the cost of handling, etc., and indeed to show some profit upon the venture of buying and using the fertilizer. So far in the development of the fertilizer industry, costs of production have risen sharply with attempts to attain standards of purity greater than 97 per cent or 98 per cent. Impurities must therefore be tolerated for economic reasons. However, as will be seen later, many of these impurities are actually beneficial. The main thing is that any impurity associated with some economic source of nitrogen or phosphorus or potassium should not be toxic to the soil or to plant life.

An aspect of fertilizer materials increasingly important today is their *physical condition.* Farmers are applying fertilizers more and more with mechanical drills, and these drills—especially those combining fertilizer and seed applications in one operation —impose stricter demands upon dryness and friability than the older methods of broadcast application by hand or by contrivances like tennis-court markers. Some materials that chemically are excellent suppliers of one or more nutrients are badly handicapped in use because they absorb water readily and cake into hard lumps, or become sticky and even fluid.

There is an important physico-chemical factor tied up with the effect of soluble fertilizers upon the soil solution. It will be well known that overdoses of fertilizers, e.g., sulfate of ammonia in heavy applications on lawns to kill weeds, can cause scorching and even the rapid death of plants. This is not a parallel with the cases of substances like arsenic and strychnine which in minute amounts are medicinal but lethal in larger amounts. It is no specific function but one that is general to all substances that dissolve in water. To understand what happens, the plant's method of drawing in moisture must be considered.

This method depends upon the phenomenon known in physical chemistry as *osmosis.* All solutions possess a pressure called osmotic pressure. A solution consists in billions of dissolved molecules, or more usually, the molecules split up into ions, rushing about in all directions within the space of the solvent. The pres-

sure can be regarded as the total effect of the collisions of these moving particles upon any dividing wall, e.g., a membrane, enclosing the solution. The pressure is a steady force for any unchanging solution for—with so many particles rushing about—the number that hit each unit area per second is a steady average figure. The osmotic pressures of specific solutions under the same conditions can easily be measured as fixed numerical properties of the substances concerned. At the same temperature and atmospheric pressure, a 1 per cent solution of, say, copper sulfate in water will always exert the same osmotic pressure.

Now this applies to the sap solution of plants. Within the cells of the root-system, that is, within skin-like membranes, the sap solution exerts an osmotic pressure according to its concentration of dissolved substances. Outside, on the other side of the cell walls, the soil solution is also exerting a different osmotic pressure according to its concentration. These cellwalls are permeable to water. Unequal pressures on either side of a permeable barrier cannot produce a static equilibrium. Therefore, water flows *into the solution with the higher pressure,* which is then reduced by the dilution, and the other solution, becoming more concentrated, rises in pressure. In this way, the two pressures tend to equalize. It is through this effort to reach a final and static equilibrium that the plant obtains water from the soil. Equilibrium is never reached for the upper part of the plant interferes by drawing much of the water up for its own needs, so keeping the sap-solution always stronger than the external soil solution. The process is thus continuous. A standstill is never reached.

The addition of soluble materials to the soil, whether they are plant nutrients or inert to plants, causes the osmotic pressure of the soil solution to rise. For normal applications of fertilizers, this does not matter much for the sap solution will always be sufficiently concentrated to exert a higher osmotic pressure. However, with overheavy applications a point can be reached when the soil solution's pressure equalizes or exceeds that of the sap solution. Then water-intake by the plant ceases or reverses with obviously disastrous consequences.

It follows from this that a material containing, say, 5 per cent of soluble nutrient, but associated with 15 per cent or 20 per cent of some other soluble, but non-nutritional, matter, will not be a very effective fertilizer. For every increase in osmotic pressure due to the nutrient, there will be three or four times as much increase for the useless associate. To turn this material into a good fertilizer, some degree of purification is necessary, by which most of the useless material is removed and the percentage content of the useful nutrient increased.

These considerations apply, of course, only to water-soluble types of nutrient suppliers. Materials whose fertilizer value is released gradually in the soil by the steady dissolving action of the soil solution clearly do not induce big jumps in the osmotic pressure on the soil side of the plant. As a subsidiary point while we are on the subject, it is clearly dangerous to give heavy fertilizer applications at times of drought when the actual amount of water in the soil is limited, for this may well make it more difficult for the plants to obtain the water they require. The use of liquid fertilizers (concentrated solutions of NPK nutrients to be diluted) for top-dressing market-garden crops is being increasingly advocated. The drought problem of supplying large amounts of nutrients during the summer growing and maturing period to onions and tomatoes, for example, is thus overcome by presenting the nutrients in solution form so that the osmotic pressure of the soil solution is not seriously increased. The problem is likely to be even more marked for cropping under glass, either in a greenhouse or with bell jars. In short, whenever the water content of the soil is abnormally low, fertilizers of a readily soluble kind should be applied cautiously and with intelligent precautions against the disturbance of Nature's osmotic arrangements.

There is a further general classification of fertilizers to be mentioned before proceeding to individual cases. They must be divided into the *inorganic or chemical* class and the *organic or natural origin* class. Of course, all materials are in the strictest sense chemical whether derived from once-living matter or from

comparatively *dead* sources. Also, all strictly organic substances are not necessarily derived from natural or once-living material—urea and calcium cyanamide are organic chemicals now synthesized by industrial processes. This classification uses the words *chemical* and *organic* in their popular rather than scientific sense. They express the *difference in origin* between the nitrogen in sulfate of ammonia produced in the gas-industry and the nitrogen in dried blood produced by treating slaughter-house wastage. Though, indeed, to split very old hairs, the nitrogen gasworks ammonia has come from a once-living source, from coal. The distinction therefore depends to some extent upon the directness and distance of the linkage between the fertilizer material and some past form of life.

The following are the more common fertilizers used:

Nitrogen Suppliers:

Sulfate of ammonia (20.6 per cent) Nitrate of soda (16.0 per cent) Nitrochalk (15.5 per cent)	Inorganic or *chemical*
Dried blood (about 12 to 14 per cent) Hoof and horn meal (about 14 per cent)	Organic

Phosphoric Acid Suppliers:

Superphosphate (17 or 18 per cent) Basic slag (9 to 18 per cent) Triple superphosphate (40 to 48 per cent) Ground phosphate rock (27.5 per cent insoluble form)	Inorganic or *chemical*
Bone meal (20 to 24 per cent with 3 to 4 per cent N) Steamed bone flour (27.5 per cent with about 0.8 per cent N)	Organic

Note. The above two bone-derived fertilizers ought to be classified with those supplying both phosphorus and nitrogen, but they are so predominantly phosphatic that it is less misleading to classify them here.

Potash Suppliers:

Muriate of potash (50 to 60 per cent) Sulfate of potash (48.5 per cent) Potash salts (20 to 30 per cent) Kainit (14 per cent) Flue dusts, especially those from blast furnaces (5 to 10 per cent)	Inorganic or *chemical*
Wood ashes (very variable with 2.5 to 5 per cent as a good average)	Often called *organic,* but this is wrong for all organic matter has been destroyed in combustion

Double Nutrient Suppliers:

P and N:

Ammonium phosphate (11.0 to 15.6 per cent N, 31 to 48 per cent phosphoric acid)	Inorganic or *chemical*
Fish products (9 to 14 per cent N, 9 per cent to 20 per cent phosphoric acid)	
Meat and bone meal (3 to 7 per cent N, 9 to 16 per cent phosphoric acid)	Organic
Guano (variable, can be 10 to 14 per cent N, 9 to 11 per cent phosphoric acid)	

N and K:

Chilean potash nitrate (14 to 15 per cent N, 14 to 15 per cent potash)	Inorganic or *chemical*
Nitrate of potash or saltpeter (about 12 per cent N, about 40 per cent potash)	

Treble Nutrient Suppliers:

Actually natural guano often contains about 2 per cent of potash, so that this could be called a complete supplier, but it was classified above as only a double supplier since it is predominantly nitro-phosphatic.

The only treble suppliers are mixtures of various fertilizer materials so designed that balanced amounts of nitrogen, phosphorus, and potassium are supplied. These are known as *compound, or complete, fertilizers.* An important part of the fertilizer industry devotes itself to this process of mixing.

These compound fertilizers can be produced with innumerable NPK analyses. They can be wholly made from inorganic materials, or they can incorporate small or quite large proportions of organic materials, and thus be semi-organic.

Here are some examples:

Nitrogen per cent	*Soluble Phosphoric Acid per cent*	*Insoluble Phosphoric Acid per cent*	*Potash per cent*
7.0	5.0	2.0	7.0
4.0	4.5	3.5	10.0
12.4	12.4	0.2	14.9
7.0	7.0	1.0	10.0

Of these four examples, the first two are part-organic fertilizers produced for horticultural applications, the second being a tomato fertilizer. The second two are entirely inorganic in composition, designed to give a high balance of nutrients to farm crops. In general, the entirely inorganic compound always has a higher analysis than the semi-organic compound, for the obvious reason that nutrient contents of organic materials are usually rather less concentrated than those of the *chemical* materials.

Lesser Used Fertilizers:

Urea (46 per cent N)	*Chemical* because manufactured, but nevertheless an organic substance in the scientific sense of this word.
Calcium cyanamide (20.6 per cent N)	Also *chemical* because manufactured, but again an organic type.
Ammonium nitrate (35 per cent N)	Chemical or inorganic.

Many by-product wastes from industries handling vegetable and animal materials, e.g., from tanneries, cocoa works, breweries, and even from modern plastics works. There are so many kinds of these materials with useful fertilizer values that it would be a long and not very useful task to attempt to give a full list.

Although detailed practical considerations about specific fertilizers are rather beyond the scope and aim of this book, we should look at some of these fertilizers more closely. Sulfate of ammonia, nitrate of soda, nitrochalk, superphosphate, basic slag, the potash mineral salts, bone meal, these are the *big names* in the fertilizer lists—we should know something about their actual origin and their mode of operation in the soil as feeders of plants. It will be clearer, if this is done under subheadings.

Sulfate of Ammonia

It is, of course, the ammonia part of this substance that matters. Originally the gas-industry offered its gas-liquor to agriculture, this liquor being the product from water-washing the gas distilled from coal. This liquor was certainly ammoniacal, but it contained very variable amounts of ammonia, it also contained certain sulfur compounds that could be toxic to plant life, and it was difficult to transport. On the whole, agriculture did not respond enthusiastically to this opportunity, so the gas makers had to think again. They treated their liquor with acids, thus forming the ammonium salts of these acids; and these salts were then separated by crystallization. Ammonium chloride was always more expensive than ammonium sulfate, so that eventually the gasworks by-product ammonia was almost entirely turned into sulfate of ammonia. The price was too high for general use in early days, and even the crystallization process did not pro-

duce a material of reliable purity. However, as time went on, the product became less varied in nitrogen content and the price also fell to levels that farmers could accept with advantage. Today sulfate of ammonia is a very standardized article, almost always containing 20.6 per cent nitrogen.

It is crystalline and normally white in color, but this whiteness is apt to be faintly tinted to various colors by minor impurities. This is a small point, but worth mentioning because inexperienced users often think they are receiving an inconsistent material when one batch has a different tint from another. It should be stored in dry quarters for moisture will tend to make the small crystals *pack* together into a hard mass which is then difficult to handle. Stacking bags heavily one upon another will also encourage this caking.

The action of sulfate of ammonia in the soil should be clearly understood. It is soluble, and the nitrifying bacteria quickly convert the ammonia-nitrogen into nitrate-nitrogen, though the readiness of their activity will be much reduced by cold weather. It has been argued that in cold wet weather, sulfate of ammonia will be wasted because the plants cannot compete with the tendency for water to wash the fertilizer away, since the nitrogen is not quickly then turned in a plant-available form. This is a pessimistic deduction not very much supported by tests. Ammonia-nitrogen seems to be held in the soil fairly well for it acts as a base, and both mineral and organic base-fixing components of the soil will *fix* ammonia. The drainage waters from soils at Rothamsted have shown that lost nitrogen is always largely in the nitrate form; thus, in several experiments, the ratio of nitrate to ammonia nitrogen in drainage water were 33:1, 100:1, 40:1, 62:1, and 77:1. These figures indicate that ammoniacal nitrogen is reasonably well held by the soil.

A more important aspect of the action of this fertilizer is its effect upon soil acidity. The ammonia part of the chemical compound being taken by the bacteria and then by the plant, the sulfate part is left free. This must attach itself to some other base, and it will seize upon the free lime in the soil to form cal-

cium sulfate. Now it is only free, unattached lime that helps to reduce soil acidity—once the lime is firmly combined it ceases to exert any alkalinity. Therefore, a secondary effect of the use of sulfate of ammonia is an increase in soil acidity.

Recently there have been attacks upon this fertilizer because of its acidifying tendency. Certainly this tendency is usually a disadvantage but it has been long known and long admitted by science and fertilizer commerce alike. Users have been told that liming must accompany regular use—indeed, it can be said that this has been drummed into the users of sulfate of ammonia. Yet only a few months ago, in a responsible journal, one of the antifertilizer school triumphantly claimed to have established a case against chemical fertilizers by stating the rises in acidity in orchard soil following the annual use of sulfate of ammonia. All that this proved was that the owner or manager of the orchard had misused the fertilizer, had not also dressed with lime or lime carbonate. Similarly, for lawn or sports grass where worm-casts are a nuisance, sulfate of ammonia *without* complementary liming is widely recommended; as the short grasses that are desirable can tolerate moderate acidity, whereas worms are discouraged by acidity. This antiworm recommendation has been seized upon by certain advocates of humus manuring as a *proof* that sulfate of ammonia drives away the earthworms and thus destroys their valuable fertility contributions. But it is only a proof for conditions of (1) regular annual use, and (2) no additional liming.

In addition to the actual capture of *free* lime, there is a loss of active calcium—for the calcium sulfate formed is more liable to be leached out of the soil. Where sulfate of ammonia has been used without occasional liming, serious declines in the content of active (or exchangeable) calcium have been measured. But, as with the acidity, liming prevents this.

Therefore, unless some degree of soil acidity is wanted (such as for lawn grass, golf courses, or acid-liking crops like potatoes), sulfate of ammonia must have this defect corrected regularly. About half its weight of lime or all of its weight in lime car-

bonate should be added to the soil at a convenient but different time in the program.

Nitrate of Soda

For nearly three centuries it has been known that the coastal areas of Chile and Peru have rich deposits of impure nitrate salts. From time to time it was observed that applications of these increased crop-growth, but no real organized development took place until the middle of the nineteenth century. After a hesitant start, the South American export of nitrates grew rapidly. The natural raw material varies in content of sodium nitrate, but the deposits are dissolved in steam-heated water, and the saturated solution thus obtained is then allowed to crystallize on cooling. This is a well-known method of purification in chemical industry for different chemical substances crystallize at different points in the process: in this case, it produces a very good commercial standard of purity for nitrate of soda, about 96 per cent purity or even more. The impurities generally contain a little iodine as sodium iodate, and some nitrate of potash, both of which can be useful—certainly the potash even if there is not much evidence for the view that iodine is a minor plant nutrient.

It is produced as small crystals. It absorbs water rather more readily than sulfate of ammonia. It is very soluble, and, with its nitrogen in the nitrate form, its effect upon crop growth is rapid and independent of the nitrifying bacteria. Since the removal by the plant of the nitrate part leaves behind a free base in the sodium part of this compound, nitrate of soda is base forming. It is about a third as base forming as sulfate of ammonia is acid forming, so a mixture of three parts of nitrate of soda with one of the other would provide nitrogen without much change in the soil's acidity or basicity.

The regular use of nitrate of soda on clay soils has been found to make them much less easy to handle—the clay becomes stickier. This is due to the sodium, which tends to enter the clay complexes, the silicates; and sodium clays happen to be much stickier

than calcium clays, etc. Continual use of this fertilizer on heavy soils, therefore, is not recommended.

The main disadvantage of nitrate of soda, however, must be associated with the easy loss of nitrate nitrogen by leaching. It must be used only when a crop can actively consume the flush of nutrient given. Thus, for top-dressing market-garden leafy crops such as lettuce, it is probably unrivaled. Its application to a crop in the seed or early seedling stage in wet weather would be less commendable. Since it is readily leached, which means that it is not held in the soil by any friendly complex, the nitrate passes downwards fairly quickly. For this reason, it often gives superior results to other forms of nitrogen fertilizers with deep-rooted crops, the roots reaching for the descending nitrate and thus making more extensive growth in their very effort to obtain food. That, at any rate, is Sir Daniel Hall's interpretation of the following Rothamsted test results, where the same amount of nitrogen was applied in ammonia form and in nitrate form year after year to wheat (deep-rooted), barley (shallow-rooted), and mangolds (deep-rooted). Over 22 years the average yield for wheat was 23 per cent higher for the nitrate; over 51 years with barley it was 3.3 per cent higher; over 27 years for mangolds it was 21 per cent higher.

Nitrate of soda today is regularly distributed at 16 per cent nitrogen. It is dearer than sulfate of ammonia, and as it contains less nitrogen this means that the price difference is even greater per unit of nutrient.

Nitrochalk

This fertilizer is an excellent example of a truly *chemical* fertilizer. It is entirely designed by man's ingenuity. It is not a synthetic product based upon some natural pattern—it is wholly an invention. It is not a true chemical compound but a mixture—a mixture of ammonium nitrate and calcium carbonate, prepared in what is called *granular* form. The ammonium nitrate is made from chemically fixed air-nitrogen, is mixed with calcium car-

bonate, and the mixture is then converted into small granules like little pellets.

The nitrogen exists in both the ammonia form and the nitrate form, and there is an accompanying and useful lime value; for calcium carbonate, though a combined form of lime, is a form that will break up in the presence of quite weak acids and act as if it were free lime.

Ammonium nitrate alone is almost impossible to handle because it so readily absorbs moisture, not merely caking but absorbing water so much that it soon becomes a solution. By mixing it with calcium carbonate (chalk) this tendency is suppressed considerably; though great care has still to be taken even with the granulated nitrochalk. If it is left about, moisture will be slowly absorbed; and, once the granules cake, not only is the mass hard to handle, but in the presence of moisture the chalk reacts with the ammonium nitrate and drives off ammonia. Nitrochalk is distributed in special damp-proof sacks to prevent this moisture damage in storage and transport.

It is a comparatively new fertilizer, introduced within the last 15 years. It is standardized at 15.5 per cent nitrogen, and can be regarded as equivalent in fertilizer value to nitrate of soda. Many authorities consider that a nitrogen application of mixed ammonia and nitrate forms is more efficient than a similar application in either form singly. Nitrochalk will be better known to the farmer and large grower than to the small gardener, for distribution in small packs has been somewhat handicapped by the need for protection against moisture absorption. Packed in tins with tight-fitting lids, however, nitrochalk in small quantities has been distributed through shops.

Other Nitrogenous Fertilizers of Chemical Kind

The fixation of nitrogen from the air can now be performed by several different processes. Electric discharge—to induce the nitrogen and oxygen of the air to combine—forms the basis of one method: passage of nitrogen over heated calcium carbide—which produces calcium cyanamide—is another; but the method

which has brought about the most rapid expansion is the Haber-Bosch process, the development of which was stimulated by Germany's need for non-imported nitrogen during World War No. I. In the Haber-Bosch scheme, nitrogen and hydrogen, heated under pressure, are passed over a catalyst, that is to say, a substance which persuades other substances to react with each other yet does not itself apparently take part in the reaction. We cannot give space to considering this strange chemical phenomenon of catalysis, and here it must be sufficient to say that, in the presence of the catalyst, some nitrogen and some hydrogen in the mixed gas stream will combine to form ammonia, and this small amount of ammonia can be separated from the unchanged gases by passing the mixed gases through water or acid.

This method of making ammonia has given great flexibility to the nitrogen compound industry. In all industrial countries today various modifications of the Haber-Bosch process are to be found. The end-product can be varied at will by further arrangement of simple chemical changes. The gaseous ammonia can be turned into ammonium sulfate by treatment with sulfuric acid; into nitric acid by oxidation; into ammonium phosphate by treatment with phosphoric acid; into ammonium nitrate by treatment with nitric acid, itself produced from the synthetic ammonia. The world is no longer dependent upon nitrate deposits in South America or upon by-product ammonia from the gas industry or coke ovens.

The future may well show that either ammonia itself, or ammonium phosphate, will tend increasingly to dominate fertilizer supplies. Particularly in the United States chemists are developing the production of compound complete fertilizers in which the nitrogen is derived partly from the absorption of free ammonia by other components of the mixture. Ammonium phosphate also offers great scope for the design of very high analysis fertilizers. Commercial ammonium phosphate has been offered at varying analyses, but this does not mean that it contains varying impurities. Chemically, there are three possible ammonium phosphates because phosphoric acid is a tri-basic acid in which one, two, or

three ammonia ions (ammonium) may enter the combination. Actually, the mono-salt is the main one for suitability, for di-ammonium phosphate is less stable, tending to lose some of its ammonia. But some ammonium phosphates offered as fertilizers have been mixtures of the mono-salt and the di-salt. (The tri-ammonium salt is very unstable.)

Urea, which is what chemists call an organic compound, can be made synthetically today, and this too may lead to important fertilizer developments for the future. It is very pure for a commercial product, the analysis showing 46 per cent nitrogen against a figure of 46.65 per cent for 100 per cent purity. It is rather a ready water-absorber but granulation processes or its partial use as a component in mixed fertilizers will greatly overcome this physical disadvantage. The nitrogen is in what is known as the amide form, which means that bacterial action must first convert it into ammonia and then into nitrate; but it is a simple amide, unlike most complex organic forms, and since urea is very soluble the action is not long delayed. Urea has been much used in the plastics industry, and this may tend to reduce its availability to the fertilizer industry.

Today sulfate of ammonia occupies the leading position. It is the cheapest form of nitrogen because its 20.6 per cent content and its price per ton have for many years offered the lowest market price per unit (per 1 per cent) of nitrogen. On a basis of economic calculation, all other sources of nitrogen are more expensive, even when their nutrient contents are higher. Having regard to the rapidity of modern chemical development, no sensible person would dare to prophesy very far ahead. Sulfate of ammonia is a particular target of the antifertilizer argument; but the increasing domination of synthetic nitrogen processes means that the fertilizer industry certainly need not be shackled to sulfate of ammonia as the main carrier of chemical nitrogen.

Superphosphate

The history of superphosphate is tied up with bones although today it is made entirely from mineral sources of phosphate. One

of the oldest ideas of *putting back into the soil what had been taken out* was the digging in of bones. However, the use that can be made in the soil of large pieces and lumps of bone is clearly limited, for bone material, though richly phosphatic, is not soluble. A factor that brought about much attention to the use of ground bones was the use of waste bone dust from Sheffield knife factories, the dust coming from the bone-grinding operations associated with handle-making. The successful use of this material, though casual rather than planned, showed clearly that bones—to be reasonably useful to the soil—must be broken up as finely as possible.

This development was roughly contemporary with the intervention of chemists like Liebig into soil fertility problems. Liebig pointed out that the fertility component of bones was the *insoluble* tri-calcium phosphate—and he suggested that treatment of bones with acids would convert this slowly active material into more soluble kinds of calcium phosphate, the di-calcium and mono-calcium phosphates. This is a somewhat chemical matter, but it can perhaps be made a little clearer as follows: a molecule of phosphoric acid has *three* hydrogen atoms which can be replaced by metals to form salts of phosphoric acid, i.e., phosphates. When all these hydrogen atoms are replaced by calcium, it happens that this tri-phosphate is insoluble, but when only one or two are replaced by calcium, then the mono-salt or di-salt is still partly acid and it happens to remain soluble in water. Liebig believed that this chemical aspect of the matter could be of great benefit to agriculture. If the phosphate material of the bones, after treatment with acid, was soluble, it would much more quickly feed the plants. There has always been discussion as to whether Liebig actually was the first to think along these lines. In this country Lawes, the founder of Rothamsted, was quietly carrying out experiments to make acid-treated phosphate fertilizers at about the same time. To make an academic fuss about the date of this discovery is obviously a waste of mental effort and time, for what happened round about 1839 and 1840 clearly seems to be that Liebig publicly suggested the method, and that

Lawes thought about it independently, conducted private experiments, and then got on with making acid-treated phosphate and selling it.

If Liebig was the first to put the idea forward in an orthodox scientific publication, then Lawes must take all the credit for the work of large-scale development. He quickly saw that phosphates need not come only from bone materials, and he looked around for mineral deposits of phosphates. He found coprolites in Suffolk and Cambridgeshire, and he imported a phosphate ore from Spain. If the insoluble calcium phosphate in bones could be made soluble with acids, why not also the insoluble phosphate in these minerals? Lawes became not only a successful experimenter, but also a successful industrialist. In less than 20 years he had opened his second factory in the London area; in 30 years or so he sold his interests for a little less than a third of a million. And much of the money he made went to the founding and expansion of the Rothamsted research station.

Today superphosphate is almost entirely made from mineral phosphate sources. Indeed, any bone-derived superphosphate would be marketed under another name such as *dissolved bone* to distinguish it from what has now become the normal superphosphate. Before the second world war, the annual world production was 16½ million tons. Processing has steadily improved with gradual increases in the percentage of soluble phosphoric acid; today, 18 per cent superphosphate (or 40 per cent in phosphate terminology) is the usual standard. Superphosphate must not be thought of as a pure chemical compound, even if *pure* means only pure to a commercial grade standard. The word *superphosphate* is a commercial word, it is not a precise, chemical name like *sulfate* or *nitrate*. The treatment of mineral phosphates with sulfuric acid produces a mixture of soluble acid phosphates of calcium, of unchanged insoluble tri-calcium phosphate, and of calcium sulfate. The high content of calcium in various forms is undoubtedly an additional advantage of superphosphate, but it is all combined as sulfate or as phosphate and no one should imagine that this calcium can act as free lime and

reduce soil acidity. The description of this fertilizer as *superphosphate of lime* has often led users to think that they were also obtaining lime in active form, which is certainly not the case.

The physical condition of superphosphate is good. It is a dry, friable powder-like substance, not tending to absorb moisture readily like many other fertilizer materials. The heat generated in the acid treatment, and the presence of calcium sulfate (gypsum) help considerably in obtaining a satisfactory final condition.

The action of soluble phosphates in the soil is rather paradoxical. For the first thing that must happen in the soil is the formation of less soluble or insoluble phosphates! If the soil is reasonably non-acidic, then free lime in it will turn the soluble calcium acid phosphate into the insoluble tri-calcium phosphate; if the soil is acid, then aluminum and iron compounds will be much freer in the soil solution and these will react with the production of the very insoluble iron and aluminum phosphates. In the latter case the superphosphate will be wasted. Why is it not also wasted in the former case? Why is it worth while to go to all this trouble to prepare soluble kinds of phosphate which are then reverted by the soil to much the same insoluble kind we started with in phosphate rock? The generally accepted explanation is that this reversion in the soil takes place as a fine chemical precipitation—the insoluble phosphate is thus dispersed throughout the zone or space of the soil solution (near the original application). No mechanically pulverized phosphate could achieve quite such an intimate association with the soil, nor would the particles be so small in size. Thus, weight for weight, a much greater surface area is presented by the precipitated superphosphate than by finely ground phosphate rock. The process of further solution and utilization—probably carried out by the dissolved carbon dioxide in the soil solution—is therefore much more rapid.

The soil reaction of superphosphate used to be regarded as acid or acid-tending, but this is erroneous. One disadvantage of acid soil is that certain toxic elements become soluble; but, in

the presence of superphosphate, these soluble toxic elements (aluminum particularly) form very insoluble phosphates. In this way superphosphate (or any soluble phosphate material) may actually remedy one of the adverse effects of soil acidity. Also, plant consumption removes phosphate, the acidic part of the superphosphate, so that the tendency of the balance left is more likely to be antiacid than proacid. The old idea that superphosphate was acid-forming, due no doubt to the fact that its manufacture involved the use of large amounts of sulfuric acid, is therefore a fallacy. Tests on soils that have been treated with this fertilizer year after year have proved this conclusively so that practical experience once again tallies with theory.

The supply of superphosphate is naturally dependent upon the supply of mineral phosphate rock primarily, and secondarily upon the supply of acid. There are a number of phosphate deposits in various parts of the world. The United States have large deposits in Florida, Tennessee, and South Carolina; Europe's main source lies in North Africa; there are rich deposits in certain Indian Ocean and Pacific Ocean islands; and Russia has enormous sources both in Europe and Asia. A. N. Gray in his *Phosphates and Superphosphate,* estimates that the major deposits known today will, at the present rate of consumption, last 1,300 years; and there are quite a number of lesser deposits of whose possibilities little as yet is known. There is no fear of phosphate scarcity. With superphosphate supply, the greater risk would seem to arise with the acid needed in the process; at any rate the cost of superphosphate depends upon the availability of sulfuric acid as a by-product from other chemical manufacture. About 11 hundredweights of 68.7 per cent sulfuric acid are needed to produce one ton of superphosphate. On the whole, the price asked per ton by superphosphate producers has always been reasonable; indeed, at one period the trade was seriously damaged by ruthless international price-cutting. Just under 80 per cent of the total phosphate rock consumed by the world in 1938 was first converted into superphosphate before application

to the soil; the actual figure for superphosphate production in that year was estimated to be 16 million tons.

An interesting international point is that not all countries insist upon a manufacturer's guarantee of the percentage of *water soluble* phosphoric acid in superphosphate; some insist upon the *citric-solubility* figure. France, Canada, the United States, and Sweden take the view that the citric-soluble phosphoric acid is a sufficient indication of the field-value of the fertilizer.

Basic Slag

Nobody produced basic slag because it was likely to be a good fertilizer. It was a by-product dumped from the steel industry for some years before its agricultural value was envisaged. The ores used for iron and steel production often contain useful percentages of phosphorus. For cast iron, the presence of this element does not matter—for steel, it must be removed. In 1878 Thomas and Gilchrist devised a steel process in which the phosphorus was oxidized during smelting and removed in the slag. Lining the furnace with lime and magnesia, they removed the phosphorus in the form of alkaline phosphates, the phosphorus having first been oxidized to phosphoric acid by an airblast through the charge. With other matter, these phosphates rise to the surface of the molten metal as slag. Not until 6 years after the start of this process did scientists suggest that the discarded slag might, by virtue of its phosphatic content, be of value to agriculture; and then tests were simultaneously carried out at Salisbury and Durham. Both the tests showed substantial crop increases over the unmanured plots. Later it was found that slag was more effective if very finely ground and from then onwards its development as a fertilizer was assured.

The availability of the phosphoric acid in slag is not a matter of simple chemistry. Though not soluble in water, it is very soluble in weak citric acid solutions. This solubility is due to the formation in the furnace reactions of a complex phosphate combined with silica, and the presence of the silica apparently makes the phosphate more soluble. That at least is a short ex-

planation of a somewhat complicated chemical question. Basic slag is usually valued and sold upon the content of phosphoric acid that is soluble in 2 per cent citric acid; it must also be sufficiently finely ground for 80 per cent to pass through a sieve of 10,000 meshes to the square inch.

However, all that is legally required is a statement of the total phosphoric acid present, i.e., soluble or insoluble, and the amount of the slag that will pass through a prescribed sieve. This was settled by the Fertilizers Act of 1926, but most people today would regard this attitude to slag as out-of-date. Probably with most kinds of mineral phosphates, the citric-acid-soluble percentage is the true measure of soil availability; certainly this seems to be true of basic slag.

Recent developments in the steel industry have restricted the quality of slag. The original process, known as the Bessemer process, has tended to be displaced by the open-hearth process. This process still produces a phosphatic slag, the grades obtained are not generally as high as those from the old Bessemer process. For one reason, fluor-spar is added to the furnace charge to obtain better steel, but this causes some of the phosphoric acid to form a very insoluble complex compound with calcium fluoride, and so to this extent otherwise useful phosphoric acid is lost. Also, except at times of full steel production, manufacturers tend to choose ores that are low in phosphorus content. These factors have somewhat altered the basic slag position. In normal times slags of low phosphoric acid content would be of little economic value, the cost of grinding and transport outweighing their fertilizer value. In wartime, when shipping problems affect the import of phosphate rock, the low grades are of some emergency value. Slag production is limited, therefore, to Bessemer-process slag plus the limited quantities of open-hearth slag whose phosphoric acid content is worth the further work of grinding and distribution.

The action of basic slag in the soil is generally considered to be slower but more direct than that of superphosphate. There is no intermediate reversion to a finely precipitated tri-phos-

phate. The soil solution slowly dissolves the phosphoric acid, which is then directly utilized by the plant. Slag is also alkaline. Its equivalent lime content is variable, depending upon how much of the calcium is free and how much combined with very weak acids such as silicic acid. A rough guide given by most authorities is that 3 parts of slag have the liming-value of 2 parts of limestone. In England basic slag has achieved its main reputation as a fertilizer for grassland or for root crops on land subject to acid-induced disease such as *finger-and-toe.* Its use for general arable cropping has been largely limited in Britain to heavy land, though the Germans actually imported considerable quantities mainly for use on very light arable soils. Farmers often express the opinion that there is nothing like slag for clover. Clover will flourish in grassland almost as if seed rather than slag had been applied. This is due to the fulfillment by slag of the essential conditions for clover that were probably not much in existence in the grassland before the application—alkalinity and phosphate supply. The clover had been diminishing because the soil had been getting too acid for the parasitic bacteria, and also because phosphate deficiency produced through grazing and non-return of this nutrient.

On the whole, though it would seem the palmy days of high-grade slag have passed, we seem rather to have under-valued our own considerable output of this fertilizer. It is possible in future that attention will be paid to methods of up-grading open-hearth slag *after* its removal from the furnaces, in which case grades of 20 per cent such as were often produced by the Bessemer process might return. The demand for good-grade slag has been generally greater than the supply for some years.

Bone Fertilizers

As already mentioned, bone meal first came into prominence through its availability as a by-product from the Sheffield knife industry. Since then, the grinding of dried bones has been a regular practice carried out directly for bone-meal production. The fat from the bones is usually removed first by steaming.

Often the pretreatment is more severe, steam under pressure being used; then not only the fats but the gelatine, etc., is removed for glue making, and the residual bone material is softer and can be much more finely ground. This material is known as *steamed bone flour.* Its friable condition gives it a special value that is not always assessed as highly as it should be; bone meal, as the more traditional bone fertilizer, is generally more favored.

The extra pretreatment of steamed bone flour reduces the nitrogen content, but the relative concentration of phosphoric acid is increased. Bone meal varies generally between 20 per cent and 24 per cent phosphoric acid with 3 per cent to 4 per cent nitrogen. The flour runs at about 27.5 per cent phosphoric acid and a little less than 1 per cent nitrogen. In both the phosphoric acid exists as the tri-calcium phosphate so it is wholly insoluble.

Chemically, Nature has produced the same substance that has been deposited in Florida and Tunisia. Not only here but in many other countries intensive growers frequently consider that bone meal is the most effective form of phosphate supply, and the general rule of thumb explanation lies simply in the statement that it is *organic.* The scientist is soon at sea on this question. Comparative tests between bone meal and other more chemical phosphatic suppliers have not demonstrated any remarkable superiority. Nor is this the only controversial point. Many growers insist upon a coarse rather than a finely ground bone meal, when almost every scientific consideration would urge the use of the finest meal obtainable—indeed, would recommend a preference for the steamed flour. The lesser nitrogen content would seem to be a minor difference between the two bone products. Although research has not on the whole been able to justify these claims for bone meal, growers of the market-garden type will readily pay several times the price of *chemical* phosphates for bone meal. This preference at a premium has been particularly marked during the war, when bone meal has risen in price rather severely. It is sometimes claimed that the effects of bone meal are much more lasting, but even this claim has not been vindi-

cated by test comparisons. Here there is certainly a conflict between scientific views and practical views.

Even so, it is hard to understand why it is not the cheaper steamed bone flour that is preferred. Quite a long time ago now Sir Daniel Hall expressed the view that bone meal was generally overrated and bone flour generally underrated—but the anomaly still reigns in market-gardens. The demand is for bone meal, bone meal at almost any price—and, even in wartime, the flour is regarded as a mere substitute for the time being.

The nitrogen content of the bone fertilizers has certainly been rather overrated. It is small even in the meal compared with the phosphoric acid value, and as such it is unbalanced. It is a slow form of nitrogen, of complex nature and needing a long series of bacterial attacks to bring it to a simple, plant-available form. The part played by bone fertilizer in general phosphate supply tends to be overestimated because so much is written and said about this claim to qualitative superiority. In 1936, the world consumed over 20 million tons of superphosphate, basic slag, and ground phosphate rock, whereas the consumption of all organic kinds of phosphatic fertilizers was only 350,000 tons (excluding guano). Those who argue that we should remedy our phosphate deficiencies only with these natural origin materials might well reflect upon the disparity between these figures, which after all are indexes of production possibilities as well as of actual trading.

Ground Rock Phosphate

This is the cheapest form of phosphatic fertilizer, and about 10 per cent of our phosphatic needs were met by ground phosphate rock before the war. It is often used for tasks for which basic slag is also considered well suited, e.g., grassland or acid arable soils. Since it is an insoluble form, its efficiency depends upon fineness of division. As with slag, 80 per cent should pass through the standard 10,000 to the square inch mesh—indeed, some grades are offered more finely ground than this. It should be looked upon as very slowly available, and moreover it should not be imagined that all the phosphate present is eventually

credited to the soil's active stock. The success of the superphosphate industry, and the numerous attempts of scientists and inventors to find other ways in which phosphate rock can be rendered more active—these in themselves prove that the raw material leaves much to be desired as a fertilizer.

Triple Superphosphate

When phosphate rock, that is tri-calcium phosphate, is treated with sulfuric acid, the superphosphate formed is a mixture of soluble calcium acid phosphate, a little unchanged insoluble phosphate, and a fair amount of calcium sulfate. If, instead of sulfuric acid, the acid used is phosphoric acid, then much more soluble phosphate is produced and there is no calcium sulfate. This is what is known as *triple superphosphate.* Whereas the usual type of superphosphate today analyzes at 18 per cent soluble phosphoric acid, the current grades of triple superphosphate run out at 48 per cent. Per 1 per cent of phosphoric acid, triple superphosphate is more expensive, but of course this relative dearness is compensated to some extent by lesser transport costs and lesser costs of handling because nearly three times the same phosphatic value is concentrated in the same weight.

Incidentally, the phosphoric acid used in this process still has to come from a phosphate source, so that no one need suppose that this is a method by which extra supplies of fertilizer phosphate can be provided. It is nothing more than a chemical method of providing a nutrient in a more concentrated form. Where the farmer would have used, say, 3 parts of ordinary superphosphate to the acre, he would use 1 part of the triple fertilizer.

*The Potash * Fertilizers*

Though in prechemical era farming, wood ashes were often beneficially applied, and though Liebig stressed the need for potassium, it was not until the eighteen-seventies that any notable development in potash fertilizer supply took place. And

* The expressions *potash* and *soda* are sometimes used instead of *potassium* and *sodium.*

this arose from the fact that Germany, somewhat accidentally, found she had potash to sell. In Stassfurt the natural brines had long been used to make salt, but the recovery of salt became difficult, and to save the industry the Germans tried deep boring for deposits of solid salt. They found salts which were mixtures of potassium and magnesium chloride. These were regarded as inferior to rock salt and discarded. Some 20 years elapsed before a chemist worked out a process of separating the potassium chloride by fractional crystallization. It was some years again before practical testing showed that these potash salts from Stassfurt were beneficial to plant life.

It is probably true that the larger and more rotational use of farmyard manure possible in those days kept up the available potash quota in the soil reasonably well, and so—without much serious potash deficiency—the benefits of potash applications were not likely to be so marked. Certainly potash fertilizers came into the agricultural picture later than their nitrogenous and phosphatic partners. Also, in this century, the cultivation of potash-needing crops such as potatoes, mangolds, and beet has considerably increased.

In addition to the Stassfurt potash mines, there are other deposits in Alsace and in Spain. Near Europe there are potash-containing salts in the Dead Sea. There is little point in considering the details of how these crude materials are part-purified and worked up into standard fertilizers.

Kainit was originally a mixture of sulfates of potash, magnesium, and sodium; and the potash content was 12 per cent to 14 per cent. Sulfate of potash is prepared from the potash minerals by mixing with magnesium sulfate in solution and crystallizing under conditions that favor the deposition of sulfate of potash. It is usually about 48.5 per cent potash. Muriate of potash (the chloride) is the naturally found chloride, purified from other salts of non-potash nature by the usual crystallization process. The potash content attained varies between 50 per cent and 60 per cent. During the war, to save shipping space, the 60 per cent grade has been almost entirely handled, but before

the war most sellers offered the 50 per cent grade. What is called *potash salts* is the mixed chlorides of potash and soda, with potash content at 30 per cent. Kainit, with the sulfate deposits in Germany exhausted, has for some time been a mixture of chlorides and not sulfates to any appreciable extent, so it is to be regarded today as a low grade form of potash salts.

Sulfate of potash is generally looked upon as the superior potash fertilizer. The heavy quota of chloride in the other types has always been frowned upon, too much chlorine being considered deleterious to crops. However, during the war when we have had to use the chloride 60 per cent source almost entirely, tests and experience have shown that the muriate fertilizer gives satisfactory results, except that it is still found to be markedly inferior to the sulfate form in glass-house applications. Certainly the physical condition of the sulfate is usually better—it is dry and friable, whereas the muriate tends to absorb moisture slightly and form hard lumps.

The flue dusts deposited in blast furnaces contain small percentages of soluble potash. These dusts can be applied directly to the soil, or the potash can be dissolved in water and thus separated from many other components of the flue dust. The solution—chiefly of potassium sulfate and carbonate—can then be concentrated, and potash salts crystallized. In both the European wars, with the potash imports from normal sources obviously restricted by military geography, much attention has been paid to this limited by-product source. In peacetime, it is an economic question: can the cost of salvaging and treating these somewhat dilute sources at home compete with the price of imported potash salts worked up from much less dilute foreign deposits?

Organic Fertilizers in General

We have already dealt with the bone fertilizers, perhaps rather out of turn, since the organic nitrogenous fertilizers were not discussed with the main nitrogen sources. The principal difference between the chemical or mineral fertilizers and the organic fertilizers is that the former are *additional* nutrient supplies to the

soil's currency whereas the latter are returns of supplies previously taken from the soil. With the chemical fertilizer we are trying to restore the nutrient balance with new capital (or new income, however you like to look at it). With the organics, we are returning previous debits. From a world fertility point of view, we cannot hope to balance the budget with organics; from a more limited localized view, one country or one area could attempt this balancing by importing the past fertility of other areas. Thus, market gardeners—always keen buyers of organics—are regularly putting into their soil the fertility from farmland represented in dried blood, hoof and horn, bone fertilizers, etc.

Dried blood is probably the best-known organic nitrogen supplier and it varies in quality (quality being relative ease of solubility) according to the method of drying. The product dried by ordinary heating methods is not so good as that obtained by steam-drying, and this latter grade is itself inferior to the low temperature vacuum-dried crystalline product. Although the nitrogen exists in a complex form, the action of dried blood is fairly quick, which suggests that it is particularly welcomed by bacteria. Like other organic fertilizers, excess is not injurious from secondary points of view, though this does not mean that excess will not be harmful from the nutrient balance angle. It is usually about twice the price of sulfate of ammonia (the difference has been much greater in wartime, but this is abnormal), but it contains only about two-thirds the nitrogen content, round about 12 per cent or 13 per cent.

Hoof and horn meal, which is precisely what it says, is another much-valued organic nitrogen supplier. Usually a little cheaper than dried blood, it is not so rapid in action, though very good results are obtained with the finely ground material. Meat and bone meal, blood and bone meal, and tankage are fertilizers made from the treatment of slaughter-house wastage. Variable analyses are offered, but generally the nitrogen and phosphoric acid contents are well balanced. Fish meal or fish guano is similarly made from fish wastage.

In America some actual plant-growing experiments were con-

ducted to estimate the relative availability of the nitrogen in organic fertilizers. These were the figures: lower grade dried blood, 80; better grade dried blood, 92; hoof meal, 78; Peruvian guano, 90; fish meal, 77; tankage from slaughter-house sources, two grades, 74 and 81; tankage from other sources, 51 and 57. These figures express the comparative readiness with which these fertilizers can give nutrients to plants. (Chemical methods attempting to test for this availability gave fair agreement with the actual plant tests.)

If a wide view of the soil fertility problem is taken, then the question of the organics is not so important, for their nutrient content is at best only a small portion of the total nutrients lost from the soil. The losses of nitrogen by leaching and by removal in liquid sewage or by loss in farmyard manure handling are each much greater debit factors in the fertility budget than the credit factors of nitrogen in organic fertilizers. There is considerable room for improvement in the salvaging of basic material for the organics—for example, many slaughter-houses stream their blood wastage into the sewers with no attempt even in wartime to collect it and dry it. Even with 100 per cent practical salvage wherever bones or blood or skins or offal residues were handled, even this would have but a small positive effect upon the total problem of nutrient losses from the soil.

Only in a narrower view does the question of concentrated organics versus chemical fertilizers become a significant controversy. The horticulturist and the intensive vegetable grower not only in England but in America *swear by* the organics, and back their preference with premium prices. As already suggested, these heavy users of organics are in reality balancing their fertility losses on a few acres with the partial fertility losses from much greater areas of farmland. The farmer may lose some of his soil's phosphates in his cattle's bone formation, but he can rarely afford to buy enough bone meal to balance this loss. His bank account dictates the purchase of superphosphate or basic slag. On the other hand the market gardener, aiming at top

quality vegetables and at earliness of crop, frequently claims that for him the organics are vastly superior.

Now this attitude can be due to prejudice. It can be caused by custom, by market gardeners sticking to traditional organic fertilizers and not trying the chemical fertilizers; in some cases I think this is true enough, because I know of quite a number of highly successful intensive growers who use chemical fertilizers mainly. In general, I do not think the attitude is created by prejudice. At any rate for the organic nitrogen suppliers there is a rational explanation of the *prejudice,* which was demonstrated in a paper by one of the leading advocates of organic fertilizers in intensive cultivation, Mr. F. A. Secrett. In this paper, published in the January 1944 issue of *Agriculture,* Mr. Secrett recommended a pre-planting basic dressing per acre for lettuce of 8 hundredweights of hoof meal, 6 hundredweights of bone phosphates, and 1 hundredweight of potash. For the moment consider only the hoof application. At 14 per cent nitrogen this is equivalent to 5½ hundredweights of sulfate of ammonia or 7 hundredweights of nitrate of soda. Now these dressings of chemical nitrogenous fertilizers—which would be soluble—would have a serious effect upon the soil solution's osmotic pressure. A grower could only apply this amount of nitrogen in chemical form if the dressing was split into two or three time-separated dressings, whereas it is quite safe to apply the whole amount in one dressing as insoluble but slowly released hoof meal. (I should say here that Mr. Secrett's explanation of his recipe was that the nitrogen need in lettuce comes after earlier development and that hoof meal was just delayed enough in action for the release of nitrogen to come along at the right phase in crop growth.)

My opinion (strictly personal and to be blamed on nobody else) is that for nitrogen the organic fertilizers have won their reputation in the market gardens and nurseries solely because very big amounts of nutrient can be safely given in single applications.

A comment upon similar lines has been made by Dr. E. M. Crowther of Rothamsted in a 1945 publication, *Fertilizers Dur-*

ing and After the War, Bath and West Southern Counties Society. He says: "Some day it may become possible to prepare slowly acting forms of synthetic nitrogen fertilizers to supplement the dwindling supplies of concentrated organic nitrogen fertilizers and the manurial residues from imported feeding stuffs. It seems unlikely that this country will be able to afford imports of these materials, or that the poor soils of India, Africa, and South America can be indefinitely *mined* to enrich our soils. These countries will need to process their farm products, so as to retain as much protein and other nutrients for their own people, stock, and land, and to export mainly oils, fibers, and starchy foods, which carry away only elements derived from the atmosphere."

When the scientists have tried to compare organic and chemical fertilizers, they have on the whole tested the organics on the scale of nutrient supply dictated by the maximum applications of chemical soluble types; hence they have on the whole failed to include the organic's main advantage to growers in their test. Or, in short, organics have won their reputation by being usable at higher rates!

Now this argument will not apply to phosphatic types. Superphosphate does not increase the osmotic pressure of the soil solution very much, for it is precipitated almost as soon as it enters the soil solution. High rates of superphosphate application can be given to non-acid soil. I am inclined to think that here the element of prejudice has come into the picture. Because nitrogenous organics have been superior, it has been assumed that all organic types must also be superior.

It has also been argued that organics are better because they provide more beneficial impurities, all of which have come from the soil initially, e.g., trace elements, trace plant growth substances. I would not doubt the truth of this where the soil has no other intake of organic kind, but where the soil, as it always should especially in intensive cropping, receives supplies of natural manures for humus, I cannot really believe that the relatively small quotas of these additional factors in the organic fer-

tilizers make much difference. This opinion seems tenable only in a badly manured garden and such cases should not be considered in this argument.

To introduce some figures, here are test-results quoted by Hall for relative yields of four crops with two chemical and one organic phosphatic fertilizer.

Crop	*Superphosphate*	*Basic Slag*	*Bone Meal*	*No Fertilizer*
Swedes	120	116	126	100
Barley	119	121	110	100
Mangolds	114	105	111	100
Wheat	106	108	117	100

Tests in Scotland, also quoted by Hall, showed that in a crop rotation bone meal was neither superior to superphosphate in the first season nor superior when second and third season crops were grown upon the residual fertility of the soil.

Admittedly these tests consider weight of crop, and farm crops, but one would have expected more marked superiority than shown by the bone fertilizer if, nutrient value for nutrient value, the organic form is as vastly superior as market gardeners claim.

The problem may involve a deeper issue, and the only sensible attitude is to say that more inquiries are needed. There is, however, one line of inquiry that should not be regarded as useful, that is to ascribe any superiority to the humus content of the concentrated organics. The organic fertilizers may indeed possess some humus-forming material in their make-up, but the quantity that would be directly added to the soil even in a large fertilizer application would be negligible against the *size* of soil humus needs. If there is any *something the others haven't got* in the organics, it is something that is needed in very small amounts—like plant-growth substances, hormones, or vitamins—not something needed in great bulk like humus.

Extra Properties of Fertilizers

Some attention should be paid to the other substances applied incidentally to the soil when fertilizers are used. Calcium—an im-

portant plant food element—is supplied in superphosphate, nitro-chalk, basic slag, ground phosphate, bone meal and bone flour, etc. The development of ammonium phosphate means that where this is used phosphoric acid is applied without calcium as an *extra.* It probably does not matter since calcium should be well supplied in lime dressings, but it may mean that where ammonium phosphate is regularly used attention should be paid more carefully to calcium maintenance.

Similarly sulfur is provided in all sulfate types, in sulfates of ammonia and potash, in superphosphate in the calcium sulfate it contains. Here again sulfur is a useful plant food element, and the introduction of ammonium phosphate and triple superphosphate takes away this appreciable source of sulfur. The amount of sulfur applied in sulfate of ammonia to farming soils must be considerable over a few years.

Magnesium deficiency is increasingly prevalent, and this has been attributed by some authorities to the higher grades of potash fertilizers now used. Before potash salts were *worked up* to high potash contents by purification processes, large amounts of magnesium entered the soil as an associate of potash. However, the remedy is simple enough, for magnesium deficiency can be corrected by the use of *magnesia-limes* instead of wholly calcium-limes.

Manganese is a useful impurity to be found in basic slag, several per cent of manganese oxide often being present both in the Bessemer and the open-hearth product. This impurity in basic slag may well avoid the risk of a deficiency which is today either more prevalent or more frequently diagnosed.

In general then, there is a good deal to be said for not having fertilizer materials too highly purified and concentrated. Unless a genuinely deleterious impurity is removed, it may well be better not to risk the removal at the same time of other impurities whose minor effects in the past have been cumulatively beneficial. Certainly in this matter we should jump very cautiously, for we are hardly past the walking stage in our knowledge of trace-element and trace-substance importance. In sand or water culture experiments, where pure chemicals are used as nutrients, the

nutrient solutions have to include many more elements than our NPK trio. Handbooks about *hydroponics*—commercial water culture—give formulae for stock feeding solutions that contain calcium, boron, iron, manganese, magnesium, zinc, copper, as well as NPK. Rapid development toward the very pure NPK fertilizer of high analysis might take agriculture in this direction too. This would not present an insoluble problem. The fertilizer compounders would be able to incorporate the additional plant foods in their mixtures. However, there would be a time-lag between problem realization and problem solution, a time-lag of crop troubles through minor deficiencies. In any case, the farmer might well feel annoyed that he pays for purification on Monday, and then pays for materials to replace the impurities he has been spared on Saturday. He would be in a similar position to the modern white-bread eaters who, after having had their vitamin B supplies removed from wheat, find they need to purchase small bottles of vitamin B preparations. The danger of overpurity in fertilizer materials is not at all serious as yet, and it may never become so; it is, however, a point to be watched.

So much then for a somewhat condensed and superficial survey of fertilizer materials in frequent use. We cannot close the chapter yet, for the popularity of mixed or compound fertilizers is ever increasing, and we ought to know something about these.

Compound Fertilizers

Farmers often argue that they can make their own complete fertilizers by purchasing the straight type raw materials, e.g., superphosphate, sulfate of ammonia, etc., and mixing them themselves. The cost of this is certainly less than the cost of purchasing a manufactured compound fertilizer. Whether this policy is sound depends upon the value of the specialized mixer's skill, and this must be examined.

Mixing raw materials is no mere job of shoveling them together in a heap. An application of ten hundredweights to the acre works out at about two-fifths of an ounce per square foot; so homogeneous mixing is required otherwise each two-fifths of an ounce will supply differing nutrient ratios. This argument is

clearly even stronger for lesser applications like five hundredweights to the acre. One essential factor in good mixing is reasonable evenness of particle size among the raw materials; for otherwise the lumpier ingredients will segregate and roll to the sides and bottoms of the heap throughout the operations. Only those who have actually witnessed the manufacture of compound fertilizers can fully appreciate the real significance of this factor. The modern manufacturer has special machinery for mixing, and the first operation is the pulverization of the initial materials, all particles over a certain sieve-mesh size being rejected and bypassed to a pulverizing mill for reduction to the passable size before admittance to the mixing chamber.

Second, there is a tendency for chemical action between many of the straight fertilizer materials. Some of these actions will not affect nutrient values, but will affect the physical condition of the mixture; others will affect both. As an example of the latter and serious kind of difficulty, mixing sulfate of ammonia with any alkaline material such as basic slag will cause the evolution of ammonia gas, creating a loss of nitrogen and a not very pleasant atmosphere. However, many books give information about what can and cannot be mixed so no farmer need fall blindly into this trap for non-chemists.

The other kind of change is more general. Here, the calcium in one material (if soluble), e.g., in superphosphate, will react with the sulfate in another, e.g., in sulfates of ammonia or potash, calcium sulfate being formed. This change will occur more completely and rapidly if moisture is present. Now this does not affect the nutrient value of the mixture at all, but it very much affects the physical condition of the mixture. For the calcium sulfate is formed throughout the heap and, by a kind of interlocking effect, a rock-like set is created. A freshly prepared mixture may be quite friable and powdery yet it will be caked and hard 48 hours later. This set is dealt with by the manufacturer (1) by letting it happen as completely as possible, and (2) by subsequently regrinding the set mixture, after which the same change will not happen again and the mixture will stay friable. A further and additional method is the incorporation of a *conditioner*

in the mixture, usually an organic material of some fertilizing value. A good conditioner will insure no recurrence of the set by keeping the reacting materials apart by a kind of space-buffering action, and by absorbing moisture itself rather than letting the chemical ingredients absorb the moisture. The manufactured compound fertilizer is generally prepared with attention to both these methods of conditioning and it will therefore remain friable even over long periods of storage. An example of a frequently used conditioner (or dryer) is steamed bone flour.

This matter of condition has always been important, but the importance is greater today when many farmers apply fertilizers with combine drills. Then, even in damp weather, the fertilizer must flow smoothly through the drill, and it will not do this if it tends to cake or form lumps, or, worse still, if it comes out of the bags in lumps. On the other hand, if it is too powdery and dry, it will blow away to some extent in windy, dry weather. A pulverized mixed fertilizer will flow through combine drills smoothly and without frequent stoppages for cleaning the drill if the compound (a) has had its set properly broken, and (b) if about 10 per cent of a good organic conditioner has been incorporated. Some people may think that this is too precise a statement; the conditioner perhaps need not be organic. However, as this is an expression of personal experience and investigation, I prefer to limit it to facts of which I am myself aware. Too often it is said that the pulverized mixture of small particles will not always drill freely, but, with compounds made as indicated above, farmers have drilled them easily and successfully even after long storage.

However, there is another method today for insuring drillability—the comparatively new process of granulation. In this method the fresh pulverized mixture is moistened to a paste, then dried in a rotary drier under controlled conditions; small granules will then be formed. The set here is not broken at all. Instead it has taken place within the walls of each granule. It does not spread through a heap or a bag of the granulated fertilizer because it is confined within the individuality of each granule. Granular fertilizers flow smoothly through drills. It is also

claimed that granular fertilizers supply plant foods more efficiently, particularly in the case of soluble phosphoric acid. There being less surface exposed, there is a slower formation of insoluble phosphate—around each granule a zone of nutrient supply is formed to which the plant's roots will move. Unfortunately, most of the tests comparing granular and non-granular fertilizers have so far been rather badly designed tests—with the granular fertilizers placed in localized positions close to the seed but with the non-granular fertilizer broadcast over wider areas of the soil. Since we also know today that this placement of fertilizers is an important factor, it is difficult to tell whether the superiority of yield in these tests is due to the special placement or to the granular form. Tests of a more direct kind are being carried out, but until these results have been published we are not in any position to be certain that granulation provides greater nutritional efficiency. It is, however, very frequently stated that granulated fertilizers are in this sense more efficient—but this is, so far, a matter of hypothesis rather than clearly established fact.

For high-analysis fertilizers, in the making of which there is no room for a good conditioner, the argument for granulation is undeniable sheerly on physical condition grounds. For compounds in which 10 per cent of a conditioner can be incorporated, granulation is not essential to drillability; though I believe many farmers think it is because they have had poor experiences with powder-type fertilizers. There is no doubt that the granulated fertilizer has come to stay though some of the new process' popularity seems to have been won by a rather unfair devaluation of the older type's virtues. However, to go into this matter any more deeply is rather beyond the simpler scope of this book. We are, after all, considering the case for or against fertilizers—and whether they are to be powders or granules is comparatively a minor point. Granulation as yet is in its infancy—it may or may not revolutionize the fertilizer industry.

How does the farmer with his home-made mixtures stand in comparison? Clearly he can hardly match all this skill and experience with a few shovels and a concrete floor. He may think so for the simple reason that he never tests his own work. He

does not take from his farm-made mixture a handful here and a handful there to see whether chemical analysis shows homogeneous mixing. If he did, he would probably have a considerable shock. Fertilizer mixing is a specialist task for specialized equipment. Since the effect upon the crop will depend so much upon the balance of the nutrients supplied, the unbalancing caused by bad mixing will reduce the efficiency of the mixture. The set may be overcome as a serious problem if the farmer makes a fresh mixing for each day's work; but, if he is using a drill in damp weather, even this may not avoid trouble.

There is another aspect. The manufacturer often bases his compounds upon formulae involving the mixing of several ingredients, and not merely the obvious single sources of nitrogen, phosphoric acid, and potash. The resultant mixture then provides some of each nutrient (this applies mainly to nitrogen and phosphorus) at different times. There may be some ammonia-nitrogen and some nitrate-nitrogen; or some chemical-nitrogen and some organic-nitrogen; and so on. This processing cannot be imitated in home-made mixing, for the more ingredients there are the more difficult it is to secure good mixing, the more this depends upon machinery. Also, there is the problem of obtaining several ingredients in small quantities.

Here are two examples of experienced mixing skill, the first from the United States, the second from Britain.

I	*Pounds*	II	*Pounds*
Superphosphate	947	Superphosphate	560
Ammoniated superphosphate	21	Steamed bone flour	224
Urea	18	Sulfate of ammonia	336
Sulfate of ammonia	166	Dried blood	84
Nitrate of soda	63	Tankage	140
Tankage	170	Muriate of potash	224
Muriate of potash	166	Sulfate of potash	196
Dolomite (conditioner)	224	Ground cocoa shell	140
Inert filler	225	Inert filler	336
	2,000		2,240

Just how important is this ability of a compound fertilizer to provide nutrient supplies at different times during plant growth is a matter upon which it is difficult to be definite. Many plants take in a great deal of their nutrient needs at early stages of growth; yet one cannot feel that some supplies released later will not also be welcome. It is something to feel more certain about when we know many more details about the relations of plants and their soil nutrients. Recent research on some crops has shown that nutrients are steadily taken in throughout the growing period.

That consumers must pay for the extra cost of this mixing is obvious enough. The compound fertilizer is dearer than would be the separate ingredients. The point becomes, how much extra and is it worth it? Clearly the value depends upon the manufacturer's skill, his efficiency of mixing and conditioning, his ingenuity in providing a good mixture from several genuinely useful ingredients. Until the operation of the Fertilizer and Feeding Stuffs Act, there were undoubtedly many attempts to provide mixtures containing worthless or almost worthless ingredients. The Act, and more powerfully the pressure of competition, has swept these unscrupulous practices into the past. Before the war it was estimated that in the United States 2 hundredweights of mixed fertilizers were used for every single hundredweight of straight fertilizer; and here, 1 hundredweight of mixed for every 2 hundredweights of straight. The demand for factory-mixed compounds is steadily increasing, a fact which surely tells its own story. The modern farmer finds it more economical to make a single application of all his likely nutrient needs, and he does not usually find it worth while to save a little money by making an inferior mixture himself.

I should add that this account may be a little prejudiced. Bias is difficult to avoid when one's own connection with fertilizers lies in this particular province. However, an effort has been made to avoid partiality, but if this has been unsuccessful—which I cannot judge—then there must be a little discounting. One point I have left out intentionally—the question of the balance of

nutrient supply, clearly a major factor in compounding. A compound that supplies the wrong NPK balance for a crop or a particular soil is clearly a bad compound whatever its condition or ingredients. This matter is better left until we discuss the misuse of fertilizers; for it is as likely to happen with straight fertilizers as with mixed fertilizers—indeed, it is much more likely.

CHAPTER XI

CONSIDERABLE TRIFLES

"One trouble about a book on agriculture is that it is difficult to make a tidy parcel of it. The book soon begins to look like a farm, with hens on the machinery in the cartshed and seed-bags in the study." MICHAEL GRAHAM, *Soil and Sense.*

THIS CHAPTER is a miscellany. There are various topics which have some bearing upon the argument for fertilizers; but perhaps not enough solid or direct bearing for them to be included in the main argument. Yet this book would be a most incomplete account of modern aspects of soil fertility if these points were ignored. For the sake of clarity subheadings are again introduced.

Hydroponics

First, a digression to register antipathy to the name chosen for this method of cultivation. It is not original to object—almost every writer who has dealt with the subject has objected. Perhaps if enough criticisms are made, the name will be dropped before it sticks. However, it must be admitted that this kind of criticism is merely destructive for no alternative name is suggested; nor, in coming across numerous protests, have I seen a suitable replacing name. So whoever invented this ugly word can well retort that those who want a better hole had better find one.

Unfortunately the almost mystical, super-technical nature of the name suggests that the subject is one for cranks, and cranks are just the kind of people who will not help the development of hydroponics. It is in its infancy and needs the mentality of the practical market-gardener to help it along much more than

that of the gadget-lover tired of pulling wireless-sets to pieces. The few amateur hydropondiacs I have so far met have been people wrapped up in the engineering side—in the tank arranging, pipe laying part of the job. Their knowledge of plant rearing has been elementary and even less.

Hydroponics is simply water-culture on the large scale—the growing of plants without soil, with their roots stuck into solutions of the necessary chemical nutrients. For many years the scientists in their laboratories grew plants in tubes, jars, and bottles, but it was not until as recently as 1929 that anybody seriously thought of expanding this experimental technique into a practical, commercial proposition. Why this idea took so long to develop in an age when every possible novelty is rapidly exploited it is difficult to explain. Perhaps it seemed cumbersome and unnecessary when, after all, much of the food already produced from the soil was being burnt or dumped or restricted so as to keep prices up.

The importance of hydroponics from the point of view of this book is that here we have plants growing without soil, with nothing but air, light, dissolved chemicals, and water. Any success achieved by hydroponic practice is clear-cut evidence that humus is not essential to plant growth however essential it may be to the maintenance of a good soil condition. This does not imply that I believe—or suggest anybody should believe—that humus is not essential to plant growth. The phrase *plant growth* should not merely cover the successful raising of one generation of plants from seedling to maturity. It means much more than that. It means the growing of generation after generation of plants, and it means their healthy growing, and their ability to bear seed for the healthy production of the next generation. Hydroponics is a subject which only just began to be studied, and its progress so far is too slight for us yet to decide what kind of real success it has achieved. There has been too much exaggeration by somewhat unscientific enthusiasts. In America there has been considerable commercial exploitation, *hydroponic sets*

being hawked in the small advertisements like any other kind of twentieth-century press-button gadget; while the really notable progress has been somewhat quietly published in one or two insufficiently known books.

Dr. W. F. Gericke, the American scientist who first developed hydroponics, published a very full account of his work and experience in 1940. His book, *Complete Guide to Soilless Gardening,* should be read by anyone wanting to inquire thoroughly into the evidence value of hydroponics. What does Dr. Gericke report for hydroponics progress to date? The hydroponic method can produce crops at economical costs only if it allows plants to be placed much closer together than is customary in their soil cultivation. There is no limitation imposed by this on food-supply as there would be in the soil, for the roots do not need extensive, space in order to obtain nutrients—the nutrients are liberally at hand. The sole limitation is *above* the root-level—a matter of the physical lateral size of plants and of their capacity to obtain enough light although so closely packed. For this latter reason the principal development of hydroponics has so far taken place in the sunnier parts of the United States. However, there is no necessity for all the plants to be of the same kind and dual or treble or even more multiple cropping is an important possibility of hydroponics..In the soil the growing of mixed crops on the same plot is attended by many difficulties owing to different seasonal requirements of planting, handling, etc., and one crop is liable to damage when another adjacent crop has to be attended to. This difficulty does not occur in hydroponic cultivation for the plants are not fixed immobilely in soil. They are only held loosely in a bed of litter or sand over the solution of nutrients. Claims of enormous crops from hydroponics come from cultivators who have exploited these advantages—close packing, multiple. cropping, and so on. It must not be thought just from the sight of large hydroponic cropping figures that *the same number* of plants per acre have produced so much more when in water solutions rather than in soil. An experiment at Berkeley, California, recorded by Gericke, gave the following yields:

Area: 1/220 of an acre.

Multiple potato and corn cropping gave: 408 pounds of potatoes and 78 pounds of corn.

Expressed on a per-acre basis, these figures = 40 tons of potatoes and over 200 bushels of corn.

Multiple cropping of potatoes and tomatoes gave: 516 pounds of potatoes; 1,000 pounds of tomatoes.

Expressed on a per-acre basis, these figures = over 50 tons of potatoes and 100 tons of tomatoes.

Now, however much one may try to discount the fullest implication of figures like these, it is impossible to deny that there is here a powerful indication that chemicals alone, applied to plants in water via their roots, can produce enormous crops. Even the most chemically-minded enthusiast would admit that more long-term evidence is required before hydroponic data can be regarded as major evidence for chemical nutrient efficiency—but on the progress made so far the facts all seem to point in one direction.

Gericke estimates—from experience and not from any kind of wishful hoping—that hydroponics will produce crops that are from four to ten times the average crop of soil culture from the same area. The reason is simple: "water and plant food can always be made available in the amounts needed." To quote him again: "A cubic foot of nutrient solution provides about six times the amount of water and nutrients contained in the solution in a cubic foot of soil." Also: "The plant-food requirement is lower in hydroponics than in agriculture because full use is made of all the nutrients provided. In soil production a considerable part of the fertilizer applied is lost through leaching or by reacting with the earth to form insoluble compounds."

One or two points that are perhaps rather obvious should be made. First, the nutrients supplied are not only NPK nutrients. The other nutrients that would normally be provided by the soil satisfactorily without additions have to be supplied, e.g., calcium, boron, iron, manganese, magnesium, etc. Also, the root system developed by the plant when given hydroponic conditions is of

a different kind from its natural soil system, for roots evolve their growth according to the ease with which they find food provided. The mechanical needs of the plant are satisfied by holding the upper root and the lowest part of the stem in a *seed bed* or *litter bed* over the solution. This holds the plant up, and the roots then grow downwards into the nutrient solution. The air needs are satisfied by aeration from the air space in the bed and in the fixed gap between bed and water. In many arrangements the litter of the so-called seed-bed is of an organic nature—straw, peat, and soil itself being used. It may be argued from this that some humus is supplied, but very little humus would be thus available in relation to the very high rate of cropping often attained, and in any case hydroponic growth has been satisfactorily recorded where seed-bed materials have been quite inert from the organic angle or when they have been composed of very fresh organic material hardly describable as humus.

In this country, Professor Stoughton of Reading University has found that hydroponic tank growth is not comparable in efficiency with soil culture. This is not a clash of observation with Dr. Gericke—it is due to climate differences, the English growing season being of much lesser light intensity than that of those favored parts of the United States where hydroponic successes have been demonstrated. Results comparable with those of soil-cropping have been obtained by sand-cultural methods, the plants being grown in sand or fine cinders or peat and the nutrient solutions being drip-fed or surface-watered. For full details a paper in *Agriculture,* June, 1942, should be consulted; but it would seem that the British climate is not promising for the kind of yields Gericke reaches in California, and at the best the method can but equal soil cultivation results.

Both, Prof. Stoughton and Dr. Gericke, however, state that the nutritional value of crops raised in this way is definitely equal to that of soil-raised crops. Tests of produce have shown this, though chapter fifteen in this book will raise the general problem: "What kind of tests and what about unknown factors of nutrition?" Gericke, indeed, claims that the essential mineral

content of crops can be increased to nutritional advantage by hydroponic growth, the plant being the medium by which raw minerals are assimilated for our subsequent dietetic needs. Gericke gives these figures for tomatoes:

	Potash	*Phosphate*	*Magnesia*	*Sulfate*	*Lime*
Soil growth	0.99	0.21	0.05	0.06	0.20
Hydroponics	1.63	0.33	0.10	0.11	0.28

The naturalist will argue that this increased quota of minerals is *unnatural* and therefore not beneficial to man, but this attitude presupposes that man and Nature are partners in the same cause and purpose, a philosophical assumption for which there is very little evidence.

However, hydroponics is not designed or carried out to produce evidence for fertilizers. It is a self-contained scientific development which is rapidly making great strides. A possible future introduction is the use of bacteria in the soil solution in imitation of beneficial bacterial activity in soils. Professor Hogben in his all-embracing *Science for the Citizen* suggests that hydroponics has an enormous future. The solar energy in many parts of the world, e.g., hot deserts, goes to waste because there is no soil to support vegetation, but, though the irrigation of desert sands would be hopelessly uneconomic and probably impractical, it would be quite possible to place water tanks there and grow plants by hydroponics in the intense sunlight now entirely wasted. Hogben says: "It is not beyond the bounds of possibility that the Sahara may become a vast open-air factory for storing sunlight in the starch and cellulose of potatoes and artichokes. This could then be converted on the spot into power alcohol and sugar."

Some exceedingly interesting information about hydroponic practice has côme forward. In the war against Japan it became necessary to provide fresh vegetable foods on base islands in the Pacific. Hydroponic farms were set up on Ascension Island, Iwo-jima, Coconut Island, and in the Hawaiian group; the soils on these islands were mainly infertile and so a method for produc-

ing food from nutrients without soil came into its own. There were some tough obstacles to be overcome; for example, on Ascension Island there was no rain to supply water and a plant to distill 30,000 gallons of water from brine had first to be installed; some of the hydroponic-raised plants required bees for flower pollination and a hive was imported from Brazil by air. The soilless farm on Iwojima was about two acres in area. These ventures were all conducted by the United States Army technical branches.

Even more significant are the further developments in Japan itself. Occupying American troops found that raw vegetables from Japanese soil carried germs of typhoid, paratyphoid, hookworm, and dysentery as a result of the crude systems employed year after year in returning human wastes; the United States Army medical authorities would certainly not see eye to eye with the humus school crusaders, who have so often instanced Japanese cultivation as an excellent example of *the wheel of Life.* Salad vegetables from Japanese soil were banned and, according to a report dated June, 1946, the United States Army were then setting up hydroponic farms covering a total area of 80 acres, 55 acres at Chofu near Tokio, and 25 acres near Otsu. These farms are composed of series of shallow concrete troughs each about 4 feet by 100 feet; the troughs partly filled with gravel or volcanic lava cinders, and the nutrient solution percolated through the inert bed. The experiment is being closely watched by Japanese scientists and students; it may have far-reaching influences upon Japanese economy, for Japan can use only about 7 per cent of her land area for food production despite her high population figure per acre.

This has been a very scanty account of a new branch of applied science. Nobody has learnt much about hydroponics from these few paragraphs. The interested reader should obtain Dr. Gericke's book.

The Misuse of Fertilizers

This is a somewhat diverse subject, for fertilizers can be

wrongly applied in many ways. Some of these ways are fairly obvious, and it might seem tedious to comment upon them. However, the matter is important, for in the next few chapters we are to look at some of the principal arguments against fertilizers, and it is necessary that we should first distinguish between two very different types of objection—objections to fertilizers based only upon results of their misuse, and objections based upon results of their proper use. It is at any rate my own view that most of those who are skeptical about chemical nutrients have arrived at that skepticism from experiences of misuse.

Today the relationship between farmers and scientists is getting closer, the time-lag between sound research and its willing practical acceptance is not so depressingly long, but in the past this has not been the case. Even recently, and especially where the wartime plowing-up policy has turned traditional pastureland into arable land, agricultural committee officials have sometimes been fighting a pretty tough battle to persuade farmers to put into practice knowledge that had been established over half a century ago. Consequently, when those who attack chemicals look back into the past and say: "There—that's what your fertilizers have done!" we must carefully consider whether what they really should say is not: "There—that's what happens when fertilizers are not used properly!" And there is a world of difference between the two situations.

On the other hand I do not seek to evade the argument that fertilizers are dangerous inasmuch as their misuse can easily lead to very adverse soil conditions. It is an argument. Just as the tattered spaces in European cities today are a fair argument against the aeroplane. No one seems to be very sure what the safety-valve is for aeroplanes, but the safety-valve for fertilizers is knowledge, not rule-of-thumb knowledge but clear-thinking theoretical knowledge. If the user knows what he is doing in principle, then he knows just when to stop and look up a doubtful point in some reliable book of reference or ask a sound adviser. Until every farmer knows *why* scientists believe that fertilizers are a good thing and knows what they have in mind at

the back of their recommendations, until then fertilizers will often be misused and consequently often criticized.

There seem to be two very obvious ways of misuse. First, over-application resulting in scorching or death of plants, especially of young plants. This misuse is decreasing as fertilizer experience becomes wider. But minor misuse of the same kind can occur if fertilizers are applied to very dry soil, when a much too concentrated soil solution may be obtained even from a normally safe application, or there can be localized trouble where the distribution of a fertilizer is uneven giving erratic patches of overdosage. Where fertilizers have been applied in a slap-dash manner, it is not unusual to find an emphatic nostalgia for the good old days when you just *bunged on loads of muck*. There is, of course, only one real remedy for the slap-dash user—to cut out his slap-dash mentality; a scientific instrument must be employed according to its specification.

The second common kind of misuse is the application of one plant-food in excess and the insufficient application of the others; that is to say, the error of unbalance. Early in this book it was seen how interconnected the functions of nitrogen, phosphoric acid and potash are. In chapter seven we dealt with the comparison of natural manures and fertilizers almost wholly in terms of nitrogen, but this was a matter of debating expediency—for if manures could solve the enormous nitrogen problem, then we knew they could also solve the other problems. However, nobody is going to succeed with fertilizers if they simply concentrate upon the nitrogen issue and use large amounts of sulfate of ammonia. The nitrogen thus applied will cause considerable extra leaf and stem growth, but this growth response will cause the plant to demand extra amounts of phosphoric acid and potash from the soil, extra, that is, to those amounts which would have been needed without the nitrogen applications. Also, the nitrogen cannot be efficiently used by the plant unless potash is there in a proper nitrogen to potash ratio. We have already discussed these points and it will be tedious to discuss them again. Anyone in contact with farmers will sooner or later come up

against a specific prejudice toward chemical nitrogen, especially sulfate of ammonia. "It forces the ground too much." Or, where this has happened, "the ground is ammonia-sick." Now there may or may not be a genuine case against sulfate of ammonia, but a great deal of the practical prejudice against it is due to the use of this fertilizer season after season without sufficient complementary provision for the other plant foods demanded by the extra plant-growth. The troubles are due to deficiencies of phosphoric acid or potash or lime or organic matter, all of which may have been used up faster as a result of the extra nitrogen and its effects. It depends, of course, very largely upon the soil content. A soil with a good phosphate or potash reserve may, for a time anyway, need only nitrogen, and the need for balanced applications may not become apparent for some seasons. Where this kind of situation is the case, the farmer must rely upon the recommendations of the soil scientist—even though it was seen in chapter four that the soil is sometimes a difficult patient to diagnose.

Michael Graham in his book *Soil and Sense,* bases a powerful argument against fertilizers on this point, but I think it is only a criticism of their improper use. He takes a costing-angle view. A farmer thinks he has done a profitable thing if, by using one bag of sulfate of ammonia on his soil, he is able to produce one extra bag of crop, the crop being worth more in cash per bag than the fertilizer. He points out that this is not nearly as profitable as it would seem since the soil has also lost extra phosphates and potash and lime, etc., as a result of the production of the extra crop. This is, of course, perfectly true so far as Graham has presented the facts. However, it does not seem nearly so powerful an argument if you introduce into its statement the *usual* crop-response from one bag of sulfate of ammonia. The Rothamsted report of 1929 gave the following figures for the increase per acre due to the application of 1 hundredweight of sulfate of ammonia, sufficient potash and phosphoric acid being available in the soil: potatoes, 20 hundredweights; mangolds, 32 hundredweights; swedes, 20 hundredweights; barley, 3.2 hundredweights; oats, 2.6 hundredweights; wheat, 2.5 hundredweights;

meadow hay, 9 hundredweights. In none of these cases is it merely a case of *one* bag of fertilizer being turned into *one* bag of extra crop, but into much more than this. True, the cereals do not show such a powerful increase as the roots, but it is still something better than the one-bag—one-bag ratio. Graham's criticism is sound enough as an argument that the crop increase has not come only out of the bag of nitrogenous fertilizer. These responses should give a sufficient cash return to pay for balanced additions of *all* nutrients, and still leave a handsome margin for profit.

However, this does raise two further points. Suppose the market price of the crop is not high enough and the farmer *cannot* get back the price of all necessary additions from the money received for the crop increase? In the years between the wars such a consideration could not easily be dismissed. If the foreign producer of wheat is raising it from soils into which he puts nothing back, thereby selling his crop at a low price and giving away his own soil fertility; if shipping is not very busy and ready to carry the wheat at low rates; if politicians and people in general are tied to the illusory idea of cheap food; then it is quite likely that the farmer will be inclined to buy just the one bag of sulfate of ammonia and risk no more of his capital or income in an uncertain commercial venture. Thus, fertilizers can be regularly misused as a result of agricultural depression or uncertainty. Then again, during the two periods when agriculture has been applauded and encouraged for our own belly's sake, during the two wars, the importation of fertilizer materials has been subject to the hazards and restrictions of war. At these times, when farmers have used all the fertilizers they can get in the full knowledge that every ounce of food they can provide will find a fair market (in principle anyway if not always in detail), the situation is apt to arise that, owing to these war difficulties, balanced applications according to actual needs might not be possible. For example, nitrogen being home-produced, tends to be more available than phosphoric acid and potash, both of which are largely derived from overseas sources.

Nevertheless, and getting away from the non-realistic idea that everything in the garden is lovely although there's a war on, it cannot be denied that at most times in the first war and at certain periods in the second one there has been a scramble for potash, and in many cases crops were planted without sufficiently balancing amounts of this plant-food. So, although in wartime fertilizers are used more intensively in order to produce maximum crops, we must be careful not to assume that this is inevitably and invariably an experience of the proper use of fertilizers. Crop failure or semifailure, or crop disease, cannot fairly be laid at the door of fertilizers unless it is known that the fertilizers were used correctly. A significant point is that nitrogen has never been rationed during the second war and only restricted during 3 or 4 months of one winter, but phosphoric acid and potash have. Also cattle have decreased, imported feeding-stuffs have been restricted, labor has been cut to the bone—and this situation has automatically restricted attention to natural manure production and handling.

This is rather speculative, but I suggest that there are at least grounds for supposing that fertilizers have been liable to misuse in the last 30 years, (1) because peacetime market-prices for produce have been uncertain or low and fertilizer purchase has been a matter of cutting coats according to incalculable cloth, and (2) because in wartime fertilizer supplies have been variably restricted. I do not mean to suggest by this that fertilizers have everywhere and at all times been used without balance in these periods. I mean only what is said—that there have been external circumstances tending to affect the practicability of balanced use. Therefore, when the fertilizer case is assailed by those who sweepingly refer to many failures in general farming, we should bear in mind that some of these troubles may be due to wartime emergencies or to long years of depression. And these matters, though they certainly affect the soil and the farmer, are somewhat outside the scientific argument. Add to this the fact that many farmers do not know a great deal about fertilizers and use them haphazardly whatever the circumstances, and it can be

seen that, taking the whole of farming generally, one can expect to find plenty of cases of unbalanced applications. All of which boils down to the same conclusion, again—each individual case of accused failure must be carefully assessed to see whether it falls into the class of genuine failure after proper fertilizer practice, or only into the class of results from misuse.

In normal times, and we can only hope that there lies before agriculture a long period in which the word *normal* implies some degree of prosperity and certainty, the use of the compound or balanced fertilizer is undoubtedly the best precaution against unbalanced nutrient supply, and this is being widely realized for the sales of this type of fertilizer are expanding at a greater rate than those of other types. More and more of the country's total fertilizer consumption is first passing through the factories of the compound manufacturers. No farmer should suppose that because a fertilizer is a compound it is necessarily perfectly balanced for his particular needs. He should do his best to decide what sort of balance his soil and crop need, seeking impartial advice or trustworthy commercial advice if he is in doubt, and he should then ask for that kind of balance and insist upon getting it. He should check the analysis of the compound he is offered to make sure that it does correspond closely enough with his needs. Soils vary in their needs and their history, and many variations in balance are ideally needed. For this reason it is to be hoped that the compound fertilizer industry will maintain its flexibility, and never degenerate into an overunified group of factories offering a limited range of standard compounds.

It may perhaps be thought from all this that the orthodox case for fertilizers is trying to have it both ways. That at one point it is said that we do not use nearly enough nitrogen so that there is annually a large nitrogen deficit; while now, at a later point, the dangers of excessive nitrogen are stressed. The danger is not of excessive nitrogen, but of excessive *unbalanced* nitrogen. Nitrogen is the primary growth-nutrient. Without adequate nitrogen the structure of the plant will not increase. If this process occurs without the accompanying efficient function-

ing of phosphoric acid and potash, the growth is unhealthy even if extensive, what the practical grower calls *soft* growth results, and this kind of leaf and stem is much more liable to pest attack and disease. Potash in particular is needed to bring about a balance and insure that the extensive plant structure is formed of healthy and efficient tissues. Where we are after leaf-food from a short-living plant such as summer lettuce, this perhaps does not matter so obviously, but where the plant has to grow for a full season and eventually produce a seed or fruit crop for our food, these derangements caused by unbalanced nitrogen become serious. Apart from the danger of pest attack and disease, the overstressing of the tissue-building function leads to delay in the other functions of the plant and the seed formation or fruit-ripening stages are held up, perhaps beyond the period when such natural circumstances as sunshine can help. Anybody wishing to demonstrate this by personal experience should see how much is lost by giving one or two tomato plants in a row applications of soluble nitrogen in the late summer. Further leaf and shoot formation will occur but the existing fruit will delay their yellowing and reddening until the autumn sunshine has departed.

Placement of Fertilizers

Considerable attention has recently been given, especially in the United States, to the position in which fertilizers are applied in relation to seeds or plants. It is rather surprising, on the whole, that more attention to this obviously important point was not paid earlier in the development of fertilizer science.

The basic fact behind differences in results that occur through variations in position is that fertilizers do not move sideways very much, but only up or down with the movement of water in the soil. Soluble nitrogen will move vertically to a considerable extent, but phosphoric acid and potash are much less mobile because the soil's tendency is to fix them in insoluble forms at some early stage in their movement and contact with the soil. Clearly, however, so far as lateral distances are concerned, the

roots of plants have to reach out to place themselves within the zone of a particle of fertilizer that is dissolving into the soil solution. To some extent this can be regarded as quite a good thing for roots to have to do. A root system that must extend itself to obtain food is thereby forced to be extensive, and provided that the point is not pressed to absurdity it is logical to suppose that a little early struggling for nutrients makes for a virile root system.

However, it is now increasingly realized that there is a lot to be said for placing fertilizers along the side of the seed in concentrated strips—what is called *strip placement.* And the provision of drills which sow seed and fertilizer at the same time out of adjacent spouts has made the operation a fairly convenient one. Some American data show that, for identical rows of seed, broadcast application spread an amount of fertilizer over 1,764 square inches of soil whereas the strip method spread the same amount over only 30 square inches. This high local concentration must be kept away from the seed or infant seedling for the resultant local osmotic pressure rise may inhibit germination or scorch the forming plant. There is likely to be sufficient protection in a quite small *lateral* distance since fertilizers do not move sideways—and by the time the roots are strong enough to reach out to this concentrated *strip* zone, the plant is not so likely to be damaged by the osmotic effect. However, quite apart from these speculations, a large number of American experiments have shown that, for similar amounts of fertilizer, this method of placement will provide bigger crop responses and more quickly ripening crops. The fact that these successes have been recorded on a numerous scale rather disposes of any counterargument that, even with a soil barrier of a few inches, the heavy concentration of fertilizer is likely to endanger the osmotic pressure equilibrium. The explanation is that, when the fertilizer is placed in a band in the soil, only the surface of the bulk of fertilizer contacts soil and soil moisture, so that *only a part* of the fertilizer is immediately active. Thus the plant is fed well and regularly throughout a long period by the one application yet without

excessive availability of fertilizer nutrients at any one time. Whereas, when the fertilizer is broadcast, it is at once in considerable surface contact with soil and soil moisture, and the soluble nutrients are more weakly, but more widely distributed; after these early stages some of the phosphoric acid and potash becomes fixed by the soil and thereby lost from any immediate point of view. Thus, so far as greater immediate efficiency of application is concerned, the virtue of strip placement possibly rests upon the fact that there is less fixation of phosphorus and potassium if there is restricted contact with the soil.

Since figures are always better demonstrations than words, here are some American test figures for beans. One thousand pounds of fertilizer per acre applied:

	Early Crop	*Later Crop*	*Total Crop*
Broadcast at planting	2,910	1,230	4,140
Strip placed	3,850	1,020	4,870
Yield with no fertilizer	2,310	670	2,980

Not only did strip placing increase the total yield, it greatly increased the crop proportion of the early yield, a factor of the utmost financial importance.

American research so far has found that no other positioning can beat the side-strip placement. Strips over the seed cause damage for the fertilizer may quickly be washed down into too early and too concentrated a contact. Similarly if placed just below, capillary upward movement of soil moisture can bring about the same effect. Deep placing below the seed will obviate this, and there are theoretical and practical indications that this also is a good position, but it is easy enough for a gardener to handle his soil in this manner for a few plants yet a much more difficult proposition for a farmer who has to plant or sow on a large scale and often in the quickest time possible.

Here are some more United States figures. Rather an elaborate set this time, and the careful way in which these factors have been investigated is nicely brought out. The crop was cotton; the fertilizer application 800 pounds of a compound per acre. The

column headed P.P.M. means parts of soluble fertilizer per million parts of soil within one half-inch distance from seed:

Placement (April 16)	*P.P.M. April 30*	*Seedlings on May 6*	*Final Yield per Acre*
No fertilizer	71	186	760
Direct contact with seed	11,819	few	95
Mixed with soil below seed	1,202	129	835
Strips 1¾ inches:			
1 inch below seed	1,335	19	122
2 inches below seed	1,173	121	371
3 inches below seed	621	173	735
4 inches below seed	337	215	1,062
Strips 1½ inches to each side:			
1 inch below seed	283	203	1,057
2 inches below seed	123	211	1,147
3 inches below seed	99	210	1,143

The important results of these tests are the differences between the measurements when the strip of fertilizer is (a) below the seed, in which case results are not good till it is 4 inches below, and (b) at the side barriered by 1½ inches of soil, in which case the results are excellent at 1 inch below and not significantly enhanced by greater downward spacing. This proves clearly that lateral placement is an effective protection against seed injury.

This kind of research work is still very much in progress. It is gradually becoming realized that the practice of placement is sound, so much so in fact that many people now go so far as to advocate *contact* drilling of cereal seed and phosphatic fertilizers. Little appreciable damage has been done to cereal seeds through close contact with fertilizers like superphosphate—at any rate, the benefits of the early abundant supply of the root-developing nutrient have been found by experience to out-weigh the small percentage of inhibited germination. Strong dressings of sulfate of ammonia or of potash fertilizers, however, should be placed so that there is lateral protection, as with these there has been found to be more risk of trouble in immediate proximity. It is, however, very doubtful whether absolute strip-contact is safe in

any combination of seed and fertilizer; some lateral protection surely seems wisest. The flexibility of the combine drill available dictates the type of placement in many cases—many drills deliver both seed and fertilizer out of the same nozzle though they are stored in different compartments. And the combine drill is a labor-saver and a time-saver. A practice not to be recommended is the pre-mixing of seed and fertilizer before drilling, the mixture then being drilled through an ordinary type of drill. This leads to difficulties in keeping the drill clean during the operation, but, more than that, the different weights and sizes of seeds and fertilizer particles cause segregation during the movement of drilling, and an even mixture is unlikely to enter the soil, however balanced the original *seed-fertilizer* mixture may be.

Another aspect of fertilizer application that should perhaps have come under the general heading of *misuse* is that of applying heavy dressings of nitrogen to autumn or winter sown cereals at the time of sowing. The nitrogen dressing should be mainly given in the spring, the crop being fed upon a phosphate diet with only a little nitrogen at most during the winter. Only in a really dry winter is it likely that much of an autumn nitrogen application will not be leached out of the soil. In a severe winter, a powerful nitrogen application in the autumn may well bring the crop on too quickly and render it liable to frost damage. The results of a large number of comparative tests have shown that in most cases a substantially better yield is obtained from the same nitrogen dressing applied in the spring than when applied in the autumn. A valuable and full discussion of this subject will be found in an article by Sir John Russell in the September, 1944 issue of *Agriculture,* the Ministry's official journal.

The Lesser Plant-Food Elements

We can now make up for concentration of attention upon humus and NPK by considering the other plant-food elements in turn.

Magnesium deficiency shows itself in the color of foliage be-

cause magnesium is part of the molecule of chlorophyll, the green pigment of plant tissues. When the soil cannot supply enough magnesium, the new growth of leaves draws upon the magnesium already in the plant in older leaves; so the yellowing symptoms will first appear on the oldest leaves. When plenty of natural manures were available, or where plenty can now be used, a sufficiency of magnesium is probably maintained. Where chemical fertilizers are heavily used without complementary F.Y.M. or compost, the magnesium reserve of the soil may become exhausted by the increased demand, for the more leaf and stem growth, the more magnesium needed for chlorophyll, and it is also possible that the fairly concentrated soil solution of such conditions does not allow the soil-magnesium supply to enter the solution so readily. Magnesium deficiency is very easily put right once recognized, for many limes are magnesia containing, and these can be used instead of normal lime; or Epsom salts (commercial grade, of course, not the human consumption purity brand), which is magnesium sulfate, could be applied. It is a serious deficiency if it is not diagnosed and remedied as it proceeds to play havoc with the health of the entire plant. Whether it is really increasing in prevalence is perhaps difficult to say; it may be that, through our increasing knowledge of it, it is now diagnosed more frequently and successfully. Indeed, this is true of all ailments and derangements—until we know what these things actually are by a name and by symptoms there is no reliable record of occurrence.

Iron is believed to be necessary not as a direct element for plant-food but as one that helps the assimilation of magnesium. Chlorophyll formation is connected with iron. Wallace considers that iron deficiency is a matter of non-availability, and that this occurs more usually in very alkaline soils. Thus, it can be caused by heavy liming. When it occurs the chlorophyll formation is inhibited severely. Since it is a deficiency caused by soil condition, the application of soluble iron salts will not correct it for these additions will also be made unavailable by the unaltered alkaline soil reactions. Wallace recommends the remedy of spray-

ing the foliage with dilute solutions of iron (ferrous) sulfate; or with fruit trees—where the deficiency more often occurs—by the drastic method of injecting solid iron salts into the stem! This last type of action may horrify those who deplore the use of *raw* chemicals from a *natural* point of view, but it must be observed that the method works and the symptoms of iron deficiency are thereby removed.

Manganese, like iron, is closely connected with the chlorophyll formation process. It has other indirect but important functions, and its deficiency symptoms can be clearly recognized by those who are sufficiently experienced. Like iron the deficiency is due to soil conditions that make it unavailable—in this case, the conditions seem to be the combined effects of alkalinity and high organic content. Wallace lists as one type of soil where manganese deficiency is most liable to occur—*old black garden soils where stable manure and lime have been regularly applied for years.* The cure, as for iron, is to spray the foliage with salts of the element in water solution. I have seen a crop of beet suffering from this deficiency in which a strip of the crop had been earlier treated with such a spray. The health of the treated strip—and the continued ill health and discolor of the remainder—was emphatic proof that both diagnosis and treatment had been correct. This was at a West-country market garden where the plant-feeding practice had been natural manures and lime to a predominant extent.

Boron deficiency can provide a number of troubles. Fruit deformation, stem hollowing, heart-rotting of root vegetables, are among them. It is very severe in its effects upon sugar beet. It is remedied by quite small applications of borax but, as excesses of boron can be toxic, expert guidance is needed when such a problem occurs. Indeed, expert opinion must be sought in all these cases of unusual deficiencies, deficiencies of plant foods or plant-growth-assisting elements which normally are readily covered by the soil's own store of them. Their diagnosis is a matter for experience, and the remedial treatments are best regarded as specialist matters.

These scanty notes are merely an indication of an expanding and complex subject. Those who would like to know more should refer to the book on mineral deficiencies by Professor T. Wallace to which reference was made earlier. There are several other elements whose insufficient presence can cause trouble—calcium, of course, though this is hardly a minor element but rather one so generally abundant that it is often looked upon as of little importance; sulfur, zinc, copper, chlorine, molybdenum. We are much less likely to have any of these troubles if we follow a balanced policy of artificial or chemical fertilizers *and* natural manures for soil fertility maintenance, for we can reckon upon the complex content of the natural manures to provide most of these materials that are so vital and yet needed only in small quantities.

Commercialism and Fertilizer Practice

It is often said that those who have chemicals to sell have harnessed science to their own interests rather than to the interests of the soil. That is to say, they have paid chemists to concentrate upon the kinds of research that deal with the effects of chemicals while nobody else has been very ready or able to foot the bill for scientific inquiries in other directions. It is also often said that the advertising pressure of large chemical firms overaccentuates the favorable claims of chemicals, and this has in a long period led to an unbalanced fashion for chemicals even among scientists themselves. A kind of fixed-idea-mentality has been built up.

From my own contacts with people who directly live by the soil and its produce, I very much doubt whether there could be any kind of humanity less susceptible either to subtle or crude advertising. Suspicion and skepticism go hand in hand with the plow and the harvester. Nature has taught the farmer far too many bitter lessons for him to be easily caught by mere man. All the same, having read and heard this argument so often, I decided to check the advertising matter issued over the past 8 years or so by one of the biggest chemical producers and distributors in the world, a company moreover with a direct pro-

ducing interest in only one nutrient of the NPK trio—nitrogen. I suppose the convention of commercial namelessness should be followed here, though few people will fail to name the firm to which I refer. I should add that I have no interest of any kind in this firm, deriving from them neither dividends nor income.

A careful survey of this firm's collected sales and advisory literature showed a consistently orthodox scientific attitude. Though it is nitrogen in which their primary interest lies, this nutrient was never *pushed* in an unbalanced way—the need for accompanying phosphates and potash was repeatedly stressed. Though having (I believe) no interest in the sales of basic slag, they had spent a good deal of their money and ability in recommending the use of this fertilizer for grassland. Though the green manure crop method for humus maintenance is the method most likely to use fertilizers, they did not press farmers or gardeners to rely upon it alone; on the contrary, recommendations for F.Y.M. and compost were much more frequent in their sales literature.

Of course a number of their leaflets dealt principally with the nitrogenous fertilizers—but, since it is this kind of fertilizer that they mainly produce, no fair-minded critic could expect anything else. Even so, unbalanced claims for these nitrogenous fertilizers were not made except possibly in announcements where the copy matter consisted in a very few words and the space had not permitted full discussion; here perhaps a critic could argue that it was said that so much sulfate of ammonia would produce so much extra crop without any reference to the extra drain of phosphoric acid and potash from the soil. This, I suggest, would be petty criticism for in all advisory matter of any reasonable length the need for balanced applications was stressed.

In literature for complete compound fertilizers, they not only gave the analyses, but in one series of leaflets they gave the full composition in terms of the different ingredients used per ton, a step much in advance of any legal requirement anywhere in the world other than in a few states of the United States.

Here are some extracts which show that the pro-chemical angle had not been allowed to dominate:

"The most successful potato growers manure their crops with dung and complete fertilizers."

"Fertilizers will help to restore exhausted grasses to vigor, but cannot give their full effect unless the pasture is rested at the right time and is therefore in a fit condition to respond."

"In every country where sugar beet is cultivated, it has been found both essential and profitable to manure the land well with dung and a complete fertilizer."

"The best rule for the amateur to follow is to apply as much dung as he can get in order to improve the physical condition of his soil, and to make up for any lack of plant food by the use of other organic and artificial fertilizers."

"It is not possible to grow well-developed healthy plants with the aid of nitrogen exclusively, whether it be applied in the form of sulfate of ammonia or any other purely nitrogenous fertilizer . . . sulfate of ammonia should be used in conjunction with fertilizers supplying phosphates and potash. . . . Supplement your work of cultivation by conserving all the trimmings from your garden, all lawn mowings, hedge clippings, dead plants, and the like, in a compost heap."

"Fertility depends on light and air, on methods of cultivation, on the presence in the soil of water, organic matter (humus), of bacteria, of nitrogen, phosphates, potash, calcium, and of small quantities of what are known as the minor elements. All these factors are interrelated so that all must be maintained at the right level if fertility is not to suffer."

This discussion has perhaps been a little off the main track, but I have felt it necessary to include it, because so frequently in books and articles that argue against fertilizers is it said that commercialism has pumped chemicals into the soil by sheer sales-pressure. And it is only fair to point out that, on the contrary, the advertising has tended to rest on a sound, scientific basis. The antifertilizer arguers may not agree with the scientific basis; indeed, they do not. However, they cannot argue that commer-

cialism has made what (to them) is a bad matter any worse. Of course, no commercial firm misses chances of *justifiably* selling its products or *justifiably* advancing claims for those products. However, fertilizer publicity and salesmanship has, at any rate in the last two or three decades, kept itself free from claims and advice which might be immediately profitable to the seller but eventually bad for the buyer and his soil.

By way of history, here are extracts from a very old-established fertilizer manufacturers' guide for farmers issued *as long ago as 1857.*

"Judiciously applied, in agriculture, artificial manures meet the natural deficiency of valuable fertilizing constituents in farmyard manures, and when both kinds are used conjointly (which we always recommend when practicable) the value of dung is greatly enhanced." "And it should always be borne in mind that these (artificial) manures are intended to supply any deficiency in quantity or quality of farmyard dung, and not to supersede its use."

The charge that those who have chemicals to sell have endeavored to persuade potential consumers to use these chemicals in maximum quantities and without relation to other factors, or that they have hired technicians to concentrate upon the chemical factor alone, cannot therefore be confirmed by studying the actual evidence. This does not rule out the possibility that in small transactions between inexpert dealers and inexpert farmers there has not often been misleading sales talk of the *blind leading the blind* variety—this sort of thing inevitably occurs. No one can expect perfection. Reputable fertilizer firms cannot be accused either of advertising unfairly or even of advertising on a very big scale at all. Besides, the commercial fact is that it doesn't pay to sell by over-selling or by getting a product misused; the poor result speaks not only for itself but over a wide area, and fertilizer producers have long been aware that they stand or fall for their continued business upon results in the field.

There is also the charge that the research work encouraged by

manufacturers—and certainly it has been encouraged for Lawes founded Rothamsted and the Jealott's Hill Station was founded by I.C.I. and many manufacturers subscribe to the costs of research stations on a generous scale—has been chemical in bias because of this kind of patronage, and that by comparison research upon fertility effects of other kinds (biological, etc.) has lagged considerably behind. I think this is well answered by a study of Rothamsted reports or by reference to Sir John Russell's treatise, *Soil Conditions and Plant Growth*. Such a study will show that an enormous amount of work has been carried out upon aspects of fertility that are not directly connected in any way with applications of chemicals. Indeed, Sir John Russell's general survey of fertility gives a rather more detailed and thorough account of the functions of micro-organisms and bacteria than most of the books that deal solely with this matter. Great scientists are not lured astray by commercialism, and the development of soil science has been continuously led by men of this caliber. The industrialists are wise enough to accept the verdicts of science and to restrict their claims and sales-talk accordingly.

I am perhaps prejudiced as a minor scientific worker in commercial harness. There it is—discount these views if you think fit. This criticism has so often been made yet not replied to, and perhaps this reply has been attempted only because of personal bias. However, the criticism that must be examined in the next chapters is much more solid, and I have wanted to remove lesser kinds of criticism, unimportant kinds, before coming to grips with principal issues. If this last argument has seemed trivial and unnecessary, then it has at least cleared the air for what are certainly weightier matters.

PART TWO

THE CASE AGAINST FERTILIZERS

CHAPTER XII

THE MODERN COUNTER-ARGUMENT

"Enthusiasm is not necessarily an enemy of thinking clearly, whilst it is indispensable for achieving great and difficult ends. The danger arises from the feeling that the passionateness of a belief provides any guarantee of its truth." The late PROFESSOR SUSAN STEBBING, *Thinking to Some Purpose.*

TO EVERY virile action there is a similarly virile reaction. The proposal that chemicals should be added to the soil met with counter-argument from the start. If other scientists could follow the reasonings of Liebig and Lawes, this was a triumph for fertilizers only upon the thinly circulated paper of technical journals. The natural reaction of the farmer was to regard the fertilizer advocate as a crank and to pursue his ancient way muttering that dung was natural and dung therefore best.

I came across, quite by chance, the following verses in an 1846 issue of the famous British weekly periodical, *Punch*. The poem—not, I fear, up to the modern standard of *Punch* from a purely literary standpoint—is a perfect historical example of the early farming reactions to chemical fertilizers. Called "A Country Carol," its theme was the disappearance of the *true* English farmer and his displacement by the *new* technical agriculturist. Here are the last three verses:

"I remember the time when the stable would yield
Whatsoever was needed to fatten a field;
But chemistry now into tillage we lugs,
And we drenches the earth with a parcel of drugs;
Makes each fallow
Physic swallow—
All we poisons, I hope, is the slugs.

Lor, when I was a youngster, who thought, to be sure,
Of guano, or gypsum, to use for manure?
Of acids and salts from the blue bottle shops—
Where we soon shall be going for tinctures and drops,
Draughts and potions,
Washes, lotions,
Pills and powders to doctor the crops.

Well, there, to myself I says often, says I,
Things will come round again, I've no doubt, by-and-by;
And your wiseacres find, arter all's said and done,
That the old plan of farming, my bucks, is the one;
Drop reliance
On their science,
Only finishing where they begun."

Perhaps the most remarkable thing about these verses from the British eighteen-forties is the close similarity of some of the phrases with the rhetorical attacks in prose that have been made by antifertilizer writers in the nineteen-forties. The line: "And we drenches the earth with a parcel of drugs," and the "All we poisons, I hope, is the slugs," will have an uncannily familiar ring to those who have read the modern and more outspoken books and articles of the humus school.

But today the counter-argument is no merely instinctive belief that dung and dung alone must settle the fertility issue. A school of skilful debaters has given new life and new shape to the old argument and they seek to prove the exclusive virtues of humus manures with a detailed case. The thesis is a twofold proposition; one part being a positive statement that humus-supplying manures are essential to plant growth and soil fertility, the other part being a negative statement that chemical fertilizers are in the long run harmful for both plant growth and soil fertility. And this antifertilizer part of the argument is presented with all the vigor and passion of a crusade against cruelty.

Unfortunately these opponents of modern fertilizer practice have assumed that the orthodox view is an unequivocal statement that chemicals can wholly replace natural manures. Most of their

guns are aimed at that target, which certainly enables them to give a dazzling display of good marksmanship, so that many people have been readily persuaded that the gunnery has been highly accurate with a notable collection of direct hits. However, what matters is not the destruction meted out to some imaginary target, but the damage done to the real target. And the real target is the orthodox belief that fertility will be maintained, and plant growth adequate for our needs, if fertilizers and manures are used as complementary additions to the soil, or, if fertilizers are used in conjunction with other suitable means of humus maintenance. The antifertilizer school rarely fires its guns at this target.

The leader of this school is Sir Albert Howard, a distinguished agricultural investigator whose personal work has mainly been carried out in India. In his book, *Agricultural Testament,* he has mobilized a lengthy case not only against chemicals as fertilizers but also against many of the practices and underlying mental attitudes of Western agriculture. For a time he preached this crusade of denunciation alone, but his ideas attracted disciples and today others are also pressing the case. Lady Eve Balfour's, *The Living Soil,* is an extension to and considerable improvement upon Howard's book. Where Howard thundered with sweeping generalities, Lady Eve pleads with detailed case-histories. Where Howard declared that chemicals must be abandoned, Lady Eve demands an inquiry to settle the issue by research . . . but by research aimed at the humus school's self-chosen target.

Now it hardly needs all this controversy to demonstrate the fact that chemicals alone are inadequate maintainers of soil fertility. Fertilizer supporters, as we have seen, say this themselves—it is part of the orthodox thesis. Even commercialists selling chemicals say so. The orthodox school and the humus school are therefore in some agreement though, on an occasion when I pointed this out in an article reviewing her book, Lady Eve in a replying article described me as a representative of *the modern chemical school.* However, there is nothing modern or new about the outlook I represent; in the first part of this book much of

the evidence behind this outlook is quite elderly and I think most scientists would agree that the chemicals-plus-humus formula is traditional and certainly not some belated kind of stop-press revision.

The humus school entirely rejects chemicals. Soil fertility must be maintained by humus manures and lime and sound husbandry—the NPK supply via chemicals must not assist. This attitude is frequently expressed in the statement that chemicals merely stimulate crops and eventually poison the soil. It is this part of the thesis that we now have to consider—the direct attack upon chemicals. That part which emphasizes the value of humus manures is, after all, only a reinforcement of evidence already examined and accepted in the first part of this book.

It is difficult for a member of one school of thought to summarize fairly the views of a much opposed school, especially, in a case like this where, as I hope to show, a good deal of the clash in thought is due not to the clashing of evidence but to the distorted interpretation of evidence which in reality does not clash at all. For example, much of the evidence brought against chemicals is evidence of what happens when chemical fertilizers are used *without* attention to the complementary provision of humus. If the humus school is satisfied to deduce from this kind of data that chemicals *poison* the soil, then the orthodox school must reply that the *poisoning* is due not to the chemicals but to the failure to look after the humus. In the original plan for this book I had intended to attempt an impartial presentation of both sides in this controversy, but (and from here onwards perhaps you had better watch very closely for the bias of which I have warned) I found it impossible to stick to this plan because, believing what I do about the essential partnership of chemicals and humus, I was unable to resist analyzing the humus school's thesis all the time I was trying to summarize it. However, for a start with honest intentions, these are the main points of the thesis:

1. Nature is the supreme farmer. Nature has never needed artificial fertilizers. We should therefore follow suit and leave

fertility maintenance to the plan of Nature, simply insuring the return to the soil of organic wastes for regeneration by bacteria, worms, etc.

2. All crop increases from chemicals are short-term benefits. Plants raised by these means are much more liable to pest and disease attacks, the natural laws of growth having been violated and disturbed. Plant disease will cure itself when plants are raised on humus manures, but plants raised by chemical help are in ever-increasing need of insecticides and further chemical treatment.

3. Comparisons of crops grown on chemicals and on humus manures always favor the latter. The nutritional value of compost-raised crops is higher than that of chemical-raised crops.

4. Nature has always insured that animal and vegetable wastes occur together, thus insuring also that they decompose together. This conjoint animal-vegetable type of humus is the type we must provide.

That, more or less, is the general thesis. Howard particularizes to the extent of insisting that the humus manure must be a compost of mixed animal and vegetable wastes. The fourth point, therefore, is one that belongs to the Howard school only. However, it will be clearest for us to take the general thesis for the moment. For, after all, if chemicals are so wrong and manures so right, then the question of which type of manure is best (that is, point 4 above) is subsidiary.

It is not necessary to repeat the quantitative estimate of manures necessary for our cropping needs, for this was done at some length in chapter seven. A figure of about 5 tons of compost and/or F.Y.M. per acre per year was arrived at both by aiming at a satisfaction of nitrogen needs in arable farming and by accepting a practical figure recorded by a leading humus school authority. It was seen from this that the total quantity nationally required, for arable and non-permanent grassland only, was, to say the very least, frighteningly formidable.

To accept the lower estimate is to accept this 5-tons-per-acre-sufficiency claim of the humus school. However, it must be ad-

mitted that the humus school has presented evidence in favor of this figure. Nevertheless, I am prepared to quibble and furthermore to quibble about the NPK values. If *all* farms were seeking organic wastes for composting, many wastes of a very much lower initial NPK value would have to go into the composts. Sewage sludges, for example, would have much lower values than F.Y.M. I suggest that, *with universal composting* in farming of all kinds and scales, much of the compost manures produced would be of lower NPK value than the average of those tested in the exploratory work of today; and, though this 5 tons per acre might be sufficient for the biological needs of the soil, it would not be sufficient for the NPK needs.

The humus school will certainly argue that the NPK needs of crops will come not only from the compost manures supplied but also increasingly from the soil's store via the increased bioactivity of the soil, i.e., fungi, bacteria, worms, etc. For the moment let this be observed: crop responses to fertilizers have shown the need for additional NPK even where humus manures have been substantially used. This could agree with the humus school's optimism *only* if it is established that fertilizers diminish or inhibit those mechanisms, biological or physical, by which locked-up nutrients are released. The point must rest at that until specific charges against fertilizers, charges of depressing beneficial soil activities, are individually examined.

The main argument for the exclusive use of humus manures can, as a practical issue, be reduced, then, to these queries.

1. Can the quantity needed be produced?

2. If this can be done, is it in any event so vital that it would not be better to adopt the less laborious policy of obtaining a more moderate quantity of humus manures and using fertilizers for supplementary NPK needs?

The millions of tons of compost manures that would be needed, that would have to be produced before anybody could say "yes" in answer to query 1, have been given as estimates in chapter seven. Even the lowest figure for a quite low cropping level is so high that many people with some claim to detailed

agricultural knowledge say that it is impossible as a universal policy.

Dr. E. M. Crowther, the Head of the Chemistry Department at Rothamsted, in a recent long article, *Fertilizers During the War and After,* Bath and West Society, makes this point: "It is quite impracticable to manufacture the large quantities of compost sometimes recommended for farms. Farmers are even loathe to cart F.Y.M. to their more remote fields, and they never succeeded in organizing the exchange of straw for F.Y.M. between arable and dairying districts, even where, as in Lancashire, the two are clearly demarcated in a single county. There is little prospect of transporting other low grade wastes over long distances."

However, if it was achieved, it would be done only by a truly colossal effort in terms of labor, transport, and planning. Effort just for the sake of effort is wasteful, akin to solutions of the unemployment problems which have one lot of men digging holes and another lot filling them up. It must be asked, therefore, whether it would not be more desirable to aim at a manures-plus-fertilizers solution. Unless there is indeed a powerful case, supported by enough concrete evidence to outweigh all the evidence given in Part One of this book, then the humus-only policy falls to the ground, even, if it could be carried out.

There is, perhaps, a third query to be raised if the answer to the first query is negative. Would it be desirable to produce only that amount of food equivalent to the total quantity of humus manures practically attainable? There is surely only one answer to this. We must have an agriculture producing at least 50 per cent of our total food needs, if not more than this. We need more food, not less. Any reduction in cropping level imposed upon us by the dropping of chemicals could be justified only by the most precise proof of the antichemical argument. This third query may seem rather trivial, but it covers a point that has occurred more than once in statements of the humus-only case—the contention that it is so vital not to use chemical fertilizers that we should be content to take from our soil just whatever

level of crop-yield is attainable from natural manure supplies.

It is the collective view of the Howard school—no chemicals at any price. They say that we *must* produce a sufficiency of manures, that there is no compromise solution, no joint use of manures and chemicals. If we need more concentrated NPK supplies, then we must use only those of organic origin—bone meal, guano, hoof and horn, etc. Those who have read one or other of the books from this school will know how strongly this attitude is expressed. For the benefit of other readers, I have borrowed the following quotations:

"The slow poisoning of the life of the soil by artificial manures is one of the greatest calamities which has befallen agriculture and mankind. The responsibility for this disaster must be shared equally by the disciples of Liebig and by the economic system under which we are living. The experiments of the Broadbalk field showed that increased crops could be obtained by the skilful use of chemicals. Industry at once manufactured these manures and organized their sale. . . .

"Mother Earth has recorded her disapproval by the steady growth of disease in crops, animals, and mankind. The spraying machine was called in to protect the plant, vaccines and serum the animal, in the last resort the afflicted livestock are slaughtered and burnt. This policy is failing before our eyes. The population, fed on improperly grown food, has to be bolstered up by an expensive system of patent medicines, panel doctors, dispensaries, hospitals, and convalescent homes. A C3 population is being created." Sir A. Howard, *An Agricultural Testament.*

"It must be remembered that the inorganic enthusiasts tend to overlook in their claims that the application of inorganic chemicals to a field crop does not so much test the effect of chemical nutrients as such, as of chemicals plus humus. That is why the humus enthusiasts demand long-term tests. Their view is that it is the presence of humus which softens the effects of inorganic chemicals, and that is only when the humus content of the soil becomes seriously lowered that the harmful effect of inorganics becomes fully apparent, although feeding value is affected

long before this stage is reached." E. B. Balfour, *The Living Soil.*

"The full value of any crop can only be secured from healthy, fertile soil, which must be treated as a living medium and not, as is too often the case, as a dead mass which can best be induced to produce a crop by repeated doses of highly concentrated so-called plant foods or artificial fertilizers, which not only rob the soil of its true fertility, but leave a legacy of pests and diseases as a measure of their efficiency." F. C. King, *Gardening with Compost.*

And here is a quotation not from a full-time member of this school, but from a practical farmer who has come under their influence. I may be unfair here in suggesting that he has formed his opinion under any influence other than that of his own experience—therefore I hasten to add that this is merely a deduction drawn from an appreciative knowledge of other aspects of his book.

"I think that our plan for the future must break the insidious grip of the advocates of artificials in the raw state. Although I would not go so far as to say that I have any tangible proof that they are wrong, I would say that if we have to use artificials to produce increase of yield, it is because we have not utilized natural resources to work really hard for us, because perhaps we have not tried hard enough to understand their power and importance." John Drummond, *Charter for the Soil.*

Now these opinions cannot be correct unless all that we have examined in preceding chapters is incorrect. Nevertheless, these writers are exceptionally sincere people, they have no shadow of an axe to grind, they are deeply concerned with the future health of the soil and the community. Why do they believe chemicals are so harmful for fertility when there are so many tests that seem to show the contrary? Is their case against chemicals no more than a modern dress version of the old instinct that muck is best? Or do they offer a sufficiency of specific evidence?

I want to avoid any statement that can be described as sweeping, but here I must take that risk and say that the amount of specific evidence is surprisingly small, while the amount of gen-

eral deductive evidence is fairly large. Now all deductive evidence depends upon the validity of the deductions, and especially must this be watched in the case of deductions from large-scale data. Sir Albert Howard's book is largely a statement of (1) a pro-humus case on sound, specific evidence, and of (2) an antichemical case based almost wholly upon this highly deductive type of reasoning. Lady Eve Balfour in her book recognizes that the antichemical thesis requires much more specific evidence and proposes plans and methods of research by which this might best be obtained.

Are there specific detailed kinds of antichemical evidence? For, if chemicals are harmful, they must be harmful for certain reasons, in certain definite ways. I think I am fairly summarizing the major humus school details here in setting them out as follows:

1. Certain fungoid reactions are of fundamental importance to plant nutrition. Chemicals inhibit the growth and development of these fungi.

2. Chemicals inhibit the activities of soil microorganisms, and thus unbalance natural fertility equilibria.

3. Chemicals similarly inhibit the activity of larger soil organisms, especially that of the earthworm.

First, then, the soil fungi department. Howard and E. B. Balfour both attach the greatest importance to associations of plant roots with fungi. Food is not taken up through the root hairs only, but also via this association, known rather uglily as the *mycorrhizal* association. Threads of fungus tissues draw their food from humus in the soil, and attach themselves rather like parasites to sublateral parts of the root system. Then, in symbiosis, which is simply a scientist's way of expressing the idea of mutual partnership or cohabitation, both roots and mycorrhizal fungus live together. The fungus digests food taken from the humus in the soil, and the plant subsequently feeds upon this food thus pre-treated by the fungus. As with legumes the air-nitrogen is first digested by the nodule bacteria, and the resultant combined nitrogen is then taken by the host-plant. Thus the

mycorrhizal fungus attachment is a means of feeding the plant.

In the past, scientists concerned in this kind of matter have been inclined to regard the mycorrhizal fungi as casual parasites inhibiting rather than helping plant development. This viewpoint has now been fairly well exploded, especially by the work of Dr. Rayner with conifer trees. Sir Albert Howard also has long been a powerful supporter of the beneficial theory of mycorrhiza.

No supporter of this theory of plant-nutrition via the mycorrhizal *bridge* would claim this to be the only way in which plants gain food. Absorption via the permeable root hairs is not discarded as an idea, but both methods of plant food movement are considered to operate at the same time. Where the humus school comes into this story is in the claim that Nature has arranged this dual method of plant feeding, and that we are bound to produce deranged and unhealthy plant growth if we concentrate only upon the method of feeding by root-hair absorption.

Now the mere statement of this theory does not of itself discredit fertilizers. Three separate points are raised and each must be proved:

1. That the mycorrhizal habit of many plants *is* vital to their nutritional mechanisms.
2. That humus is essential to mycorrhizal development.
3. Most important of all so far as the case against fertilizers is concerned, that, even in the presence of humus, chemicals (of fertilizer kind) inhibit or prevent mycorrhizal fungi growth.

The most thorough work yet carried out in this field is Dr. Rayner's conifer research, a full account of which is given in *Problems in Tree Nutrition* by M. C. Rayner and W. Neilson-Jones, Faber and Faber. These researches have been seized upon by the humus school though I think it is quite fair and accurate to say that the aim of the work was completely detached from any considerations of soil fertility controversy. What Dr. Rayner proved is that the mycorrhizal habit is vital *for conifer seedlings* and that this habit cannot be adequately developed in absence of humus supplies. This is an answer to point 1 and 2 above.

However, there is this important qualification—almost all the experiments in this work were experiments with the Wareham soil of the test plantations. Dr. Rayner and Professor Neilson-Jones are most careful in their own accounts to make it abundantly clear that their findings are restricted to these specialized conditions, i.e., conifer trees (in almost all experiments) and Wareham soil. When they suggest at the end of their book that some of their conclusions might have a wider significance, they do so with the proper caution of scientific research. In using this work as a part of the antifertilizer argument, humus enthusiasts have not been equally detached. They have made or indicated just those sweeping deductions which Rayner and Neilson-Jones did not feel justified in making.

I would not have brought into this book a fairly lengthy and complicated discussion of this specialized forestry research had it not been that two chapters in *The Living Soil* are very plausibly devoted to enrolling Dr. Rayner into the antichemical fold. I am aware that Lady Eve Balfour's reasoning in this seems impressive. The reader is given the feeling that one can proceed in a logical and straight line from the Wareham results to the death-knell of chemical fertilizers, but Dr. Rayner's work (which itself is not in dispute) can be quite differently interpreted as the next few pages will, I hope, show.

First, what kind of soil is this Wareham plantation soil? Quoting Dr. Rayner:

"The area first selected for field plots was on Wareham Heath, Dorset, where sowings had yielded poor and inconsistent results. The area is an exposed one, 50 feet to 250 feet in elevation. The soil, of very poor quality on Bagshot sands, consists of several inches of heather peat over 12 to 24 inches of bleached sand, with sometimes an overlying layer 6 to 8 inches thick of flinty gravel. Both iron pan and moor pan are locally present with marked effects on the surface drainage. . . . These heaths are subject to severe drought in summer and to local flooding in autumn and winter. The natural vegetation is very poor in species. . . ."

Now the decision to test this kind of soil for conifer growth had nothing to do with arable agriculture or with the conflict between soil fertility ideas. The research was carried out for purposes of forestry. The first point I seek to establish here is that no soil which can be described as above is suited to the conduction of any tests from which conclusions are later to be drawn in regard to normal agricultural practices. Of course chemicals must fail to help any kind of growth on soil like this. Indeed, the remarkable thing revealed in the accounts given by Rayner and Neilson-Jones is that applications of phosphatic fertilizers did indeed provide somewhat better growth. However, the only substantial and significant improvements in the fertility of this wretched soil were reached by the addition of compost manures. That was the first advance in the Wareham investigations. It is this, I think, that has led the humus school to emphasize Dr. Rayner's work so much.

Is this at all incompatible with the orthodox view of soil fertility? This soil was peat soil, non-aerated by water-logging in winter, clearly suffering from many deficiencies, but most of all from humus deficiency. It could not be expected that any imposed plant growth could thrive until first of all the main limiting factor—humus shortage—was remedied. When Dr. Rayner found that seedling growth was much better after compost application than after NPK applications, she was establishing just that point. The facts that Dr. Rayner went into the reason for this more deeply, and established (for this kind of plant) a vital connection between humus and mycorrhiza and a further connection between mycorrhiza and growth, these are separate issues. My own feeling is that many other humus-associated factors besides the purely fungal one of mycorrhiza should also be given credit for the better results obtained with compost applications. Thus, in a general sense, Dr. Rayner's experiments are pro-humus experiments, and since the soil was notably humus-deficient or humus-inactive her conclusions are entirely in line with orthodox opinion. Had she been trying to establish plantations of non-mycorrhizal plants such as cabbages or tomatoes she would

surely have found the same thing, namely that first something had to be done to improve the humus status of the soil.

Next, the mycorrhizal aspect must be considered, and here it must be borne in mind throughout the discussion that we are not dealing with vegetables but with trees, and not even with several kinds of species of trees but with the conifer species. It may be felt that here I am splitting hairs for the sake of cussedness. However, suppose this work had confined itself to various kinds of peas, and it had been the bacterial nodules upon which attention had been focused as a result of humus applications. The importance of these nodules is specific only to the leguminous kinds of plants and it would have been quite wrong at any stage of the work to assume that any conclusions reached were generally applicable to most other kinds of vegetation. I admit, it is true, that far more plants are mycorrhizal than leguminous, but we do not know, and it has not been established, that the mycorrhizal phenomenon is as vital to plant-nutrition in other forms of plant-life as it has been proved to be for conifers. Nor must the rather obvious point, that a tree or even a tree seedling is a long way off from a farm vegetable, be forgotten. No one is entitled to base sweeping generalizations upon any point that emerges from this very specialized Wareham investigation.

Dr. Rayner then proved that the untreated Wareham soil prevented mycorrhizal development upon young tree roots because the soil was toxic to this type of fungus. Further, she proved that the soil ceased to be toxic after compost addition, which allowed mycorrhizal development to begin. And, when this happened, tree growth was subsequently healthier in every respect. Also, it was shown that when the same NPK value as that of the successful composts was given in the form of chemical nutrients, the mycorrhizal development was not at all comparable, and this was shown to be due to the fact that, whereas the composts stopped the production or the effect of antimycorrhizal substances in the Wareham soil, the chemical nutrients could not stop the adverse effects of these inhibiting substances. That is to say, *it was not what chemicals did that inhibited mycorrhizal develop-*

ment, but what they could not do; or, in other words the factor preventing mycorrhizal development was in the Wareham soil from its past history, and chemicals could not remove it, but humus, in the form of compost, could.

Here are the actual words in one of the Rayner–Neilson-Jones points of summary: "The results confirm conclusions previously reached in respect to the existence of actively deleterious substances in the experimental soil. They show further that addition of organic composts puts an end to the production of such substances, whereas addition of equivalent amounts of available nutrients as inorganic salts is practically without effect."

Here we have a specific plant in whose development the mycorrhizal factor is clearly of proved and high importance. Because such plants cannot be raised in bad soil toxic to mycorrhiza until the soil has been rendered non-toxic by humus introduction, the mere correction of soil NPK deficiencies by chemicals can only induce a slight and non-lasting improvement over results in untreated soil. Compost applications, by correcting the toxic condition, give much better results for the same associated NPK supply. However, is there a vestige of antichemical evidence in this? The toxic condition of the organic substrate of the Wareham soil had no connection at all with the use of chemicals either as an experimental fact or as a historical fact. Indeed, the toxicity was purely a product of Nature!

Because it was outside the aim and purpose of Dr. Rayner's work, experiments were not undertaken to show whether mycorrhizal development was handicapped when compost plus extra NPK nutrients were used—or what would happen in later stages of conifer growth if chemicals were applied for NPK foods *after* the mycorrhizal habit had been stabilized by the initial humus introductions. Adverse results in such experiments would then have given antichemical evidence in regard to the limited conditions of (1) a pronouncedly mycorrhizal-habited plant being grown upon (2) originally antimycorrhizal ground.

In the Balfour interpretation of Dr. Rayner's work there are one or two points which are claimed as indications of antichemi-

cal significance and these should be answered. One point made is that superphosphate proved to be lethal to young conifer trees while basic slag or bone meal applications of phosphates *brought about effects in kind similar to that induced by compost treatment* though not of such magnitude. This seems on the face of it to be a differential piece of antichemical evidence, but I would suggest that superphosphate was obviously the wrong kind of fertilizer to apply to the Wareham soil, whose acidity was very high (pH about 4.5), whereas basic slag was particularly a good type of phosphate supplier to apply to acid soil. In any event nothing had been done in these cases of chemical application to remedy first of all the major humus-deficient soil condition. On soil of this infertile, anaerobic and toxic nature, direct use of chemicals and comparisons of different chemicals must be valueless; certainly it would be highly illogical to set such results up as counterevidence to the kind of evidence derived from extensive tests at centers like Rothamsted.

Another point is made of an isolated laboratory experiment carried out by Neilson-Jones. It was suspected that sulfuretted hydrogen (H_2S) was the toxic substance present in Wareham soil, this gas being possibly produced by organisms in the soil which reduce sulfates, as an unusually large number of these anaerobic sulfur-reducing bacteria were present in Wareham soil. The laboratory test was as follows: a number of soil cultures were given (a) sodium chloride, 0.1 per cent solution, or (b) sodium sulfate, equivalent strength, sufficient solution being added in each case to create waterlogged *and therefore anaerobic* conditions. After 12 days during which the sulfur-reducing bacteria had had their chance to operate, the cultures were exposed to the air for infection by airborne fungi spores. The chloride cultures all showed profuse fungus growth, the sulfate cultures all showed either no growth or very sparse growth.

Now this experiment had some guiding significance to the Wareham series of researches, but it has none for the humus school argument against chemicals. Neilson-Jones by this test proved to his satisfaction that some fungi (note, *not* the mycor-

rhizal fungi) could be inhibited by H_2S produced by the sulfur-reducing bacteria in Wareham soil working upon artificially added sulfate in anaerobic conditions. This was shown by the lack of inhibition when chloride and not sulfate was present. That was all, because the Wareham scientists had not been able to establish the actual presence of any H_2S in the Wareham soil, nor had they shown that, as with the other fungi, H_2S actually inhibits mycorrhizal growth.

To try to make this little pilot experiment carry any greater weight or support any general deductions is indeed straining it. Lady Eve Balfour quotes it in a chapter of circumstantial evidence, but I suggest that even this tentative description is beyond its strength. The statement is made that sulfates are dangerous because they encourage toxin-producing organisms which inhibit fungi. And this statement does not follow after the text of the experiment, but a little later when straw-composting is being discussed; thus, although the experiment had been described as only circumstantial in one part of the book, the argument about the danger of sulfates is later stated as a proved fact, being introduced with the positive words: "*as we have seen.*" Thus a vague and uncertain clue becomes a whole case proved, settled, and established when it crops up again a few pages later! Yet it is not proved at all. The Neilson-Jones experiment was carried out under anaerobic (airless) conditions. It was also dependent upon the initial supply of numerous sulfur-reducing bacteria in the anaerobic Wareham soil. No competitive bacterial action could occur since the conditions for sulfate-reduction were the only conditions created. How can it be argued that similar events will occur either in a compost heap or in useful soil where the conditions are *aerobic?* These reducing-type bacteria function to a significant extent only in anaerobic conditions—they obtain their oxygen by reducing oxygen-containing substances like sulfates because no other source is available to them. Yet this test is used as the basis of a positive statement that sulfate additions can inhibit fungal development *in aerobic conditions.* It is, of course, nothing of the kind.

There is, however, one piece of evidence in the Rayner experiments which can slenderly be regarded as an antichemical indication. In detailed work comparing the effects of various composts, outstanding results were obtained with those in which the added nitrogen was from dried blood, i.e., organic. I must justify the use of the word *slenderly* in the sentence above. In the various composts made, most of those which used dried blood as the nitrogenous addition differed from the inorganic nitrogen cases in the organic materials used; only in one of the dried blood composts was the original compost ingredient the same, i.e., straw. For proper and certain comparisons a reasonable number of composts *all* otherwise similar in ingredients must be made using (a) organic nitrogen and (b) inorganic nitrogen; then and then only is the general result clear and beyond dispute. For facts about these composts whose effects on the Wareham soil were compared reference must be made to the book by Rayner and Neilson-Jones (page 63). It should, by the way, be added that Rayner herself states that in the effects upon Wareham soil considerable differences were observed in the composts used according to their carbonaceous ingredients, so that similarity in this matter when comparing added nitrogen sources is no small point. However, some indication must be accepted here, and the real indication is that research of a wider kind and upon other kinds of soil and cropping in regard to compost effects should be carried out to see whether dried blood does help to make better compost than sulfate of ammonia or nitrochalk or calcium cyanamide. This type of work is very properly part of the projected research plan at the Haughley Research Farm (see later).

And now to return to the mycorrhiza. Suppose we assume a great deal more than we really are entitled to about the implications of the work on conifers—suppose we even accept the fact that chemicals would never be of much value to the conifer because of the predominant mycorrhizal factor in its nutrition. This is not proved. It is only proved that chemicals cannot help until the Wareham troubles are put right with humus from com-

posts. However, for the sake of argument, suppose it is. How far could we extend this as a law of *general* plant life?

First, all plants are not mycorrhizal. Notably non-mycorrhizal are the cabbage family and the tomato plant. Most of the important overseas crops, e.g., tea, coffee, cotton, sugar-cane, etc., are mycorrhizal; in this country, cereals, grasses, the legumes, potatoes, hops, etc., are also mycorrhizal. Second, there are two distinct kinds of mycorrhizal attachment. For most trees and shrubs the type is *intercellular,* a sheath of mycelium enclosing the root-tips, but for most crop plants, the type is *intracellular,* and the mycelium in this case is eventually digested within the cell-walls. (I must warn readers here that this kind of information would be much more clearly presented by a mycologist than by a chemist, but I hope I have given a fair and accurate statement of the differences between the two kinds.) It follows from this that whatever is true of conifer mycorrhizal development may be in no way similarly true for the different kind of mycorrhizal development associated with most farm and plantation crops.

In any case the wide extension of the principle that mycorrhizal development is so important is faced by this most awkward fact—plants like cabbages and tomatoes are non-mycorrhizal yet no one could deny that humus is highly important as a factor in their proper cultivation in soil. I do not mean that this fact knocks down the mycorrhizal argument for all the other plants, but I do suggest that it enforces a much wider view of the significance of humus than any restricted mycorrhizal view. It may well be that the mycorrhizal bridge mechanism of nutrition is far more important in large root systems such as those of trees than in the root systems of smaller kinds of plant life, where the root-hair mechanism may be much more significant because of the greater relative surface area of root-hairs (I admit to plausible guessing here) and because of the shorter life of the plant which allows less time for extensive mycorrhizal invasion. I present this suggestion cautiously recognizing that I may be in an undistin-

guished minority of one in thinking there is much in it. Anyway, it seems strange that a major *mechanism of nutrition* should be denied to some plants.

Third, if the mycorrhizal function is so important, and if chemicals are damaging it, how can all the results in favor of fertilizers be explained? Also, where does the evidence of water-culture or sand-culture (hydroponics) fit in with any fundamental view of mycorrhizal associations? These results admirably confirm the root-hair theory of nutrition but no place seems to be available for any root-fungal mechanism of feeding.

And there we must leave the mycorrhizal phenomenon, perhaps rather skeptically treated here, but to offset this I suggest a study of either the Howard or Balfour book on the humus-only case where these castings of doubt will be well counterbalanced by the most spacious homage. I would add one point in fairness to Lady Eve Balfour: she does present the mycorrhizal argument as circumstantial and it may seem that I have accused her of treating this evidence as concrete and direct. In one point I think this was done, and I have drawn special attention to it. It is the space and emphasis given to the general mycorrhizal theme which is liable to make the reader feel that it constitutes powerful antifertilizer evidence, and which in turn makes counterargument necessary.

Passing on to the second kind of antichemical charge, the charge that chemicals depress micro-organic activity in the soil, we find again plenty of pro-humus evidence, but an absence of precise antichemical evidence. So long as there is present an adequate supply of humus, there is no reason to suppose that bacteria will not perform their functions. It is true that the beneficial bacteria like non-acid conditions, and it is true also that one or two of the well-known fertilizers tend to create some acidity, but it is in any case part and parcel of fertilizer practice to compensate these tendencies by the use of lime. It is believed that the legume bacteria will not bother to *fix* air nitrogen if there is present an adequate supply of already combined nitrogen

in the soil, but in any case, it is not good fertilizer practice to supply nitrogenous fertilizers liberally to these crops.

Are the azotobacter discouraged in their useful work if fertilizers are regularly used? The measurement of the azotobacter contribution is so difficult that it is probably impossible to answer this question with any accuracy. In absence of any solid information, we must guess; and my guess is that there probably is some inhibition. In richly nitrogenous soil the leguminous bacteria are said to be lazy—therefore it is probable that the azotobacter will behave similarly. For their own life purposes they need nitrogen so badly that they have evolved this special capacity to take it from the air. It is likely that they will take the easier course and temporarily absorb soil nitrogen when it is present in liberal quantities. However, there is this to be said in the other direction: fertilizer and husbandry practice so far has generally failed to maintain a nitrogen balance, and therefore the probability is that in most practical cases the azotobacter will still have nitrogen-demanding conditions to meet, so the degree of inhibition is not likely to be severe.

In any event, the maximum azotobacter contribution in favorable circumstances has never been estimated to be able to balance even the leaching loss in arable cropping; so that, if faced with the problem of abandoning fertilizers or abandoning these independent bacteria, we should have ample evidence to justify the latter course, however much it might spiritually horrify the *natural* school. Is it possible for either side of this argument to bring the azotobacter into the witness-box? To do so, one must first be able to distinguish the azotobacter nitrogen production and measure it when several other kinds of nitrogen supply and removal are operating at the same time. Unfortunately soil nitrogen is not labeled according to origin.

Nor is this the only unknown segment of this matter. There must be many bacterial actions in the chain of soil fertility arrangements of which we are still ignorant. It is very easy for those who attack fertilizers to say that chemicals *may* disturb these mysterious but vital processes. It is, however, just as easy

to say that fertilizers *may* not disturb them. We simply do not know. Failing any specific evidence of this kind, what can we logically do but rely upon the general facts that have been discovered and established about responses to fertilizers?

In regard to the nitrifying bacteria—those that turn complex nitrogen into active nitrogen—there can hardly be a serious charge that fertilizers damage these essential workers. If that were so, then sulfate of ammonia would never be in any way effective, for its bacterial conversion to the nitrate form is a necessary stage in its utilization by plants. And we have the long-term test evidence from Rothamsted to show that nitrogenous fertilizers of this kind are, weight for weight in nitrogen, more effective than F.Y.M. in plant-growth increase. If chemicals upset these bacteria, one would surely anticipate reverse results.

When we come to the larger soil organisms, and in particular to the earthworm, the humus school stands in a stronger position. For the earthworm's contribution to soil fertility has been sadly neglected by orthodox soil science. Even in the United States where official research facilities in agriculture are so liberally supported, even there most of the modern work upon the earthworm has been left in private hands.

The scientific estimation of the earthworm's contribution begins with Charles Darwin. Over a number of years he observed worms' habits and the many kinds of soil changes they brought about, and in 1881 he published a monograph, *The Formation of Vegetable Mould Through the Action of Worms with Observations on Their Habits.* This exhaustive study was no ordinary record of a naturalist's investigation, otherwise there might be more excuse for the scanty attention paid to it by contemporary and later science. Darwin was not content to present a *purist* view of the worm—he went much beyond this and stressed the important consequences of worms' habits to the soil. However, what should have been a classic in scientific literature caused practically no stir at all. Darwin's fame was to rest upon apes, not worms. Recently, however, and in no small measure due to the activity of the modern humus school, this book has

been republished under the neater title, *Darwin on Humus and the Earthworm* (Faber and Faber), with a preface by Sir Albert Howard. Not unnaturally Sir Albert ties up Darwin's neglected points with the humus school thesis. However, before we inquire into this enrollment of Darwin as a new member of the humus school—or should it be as a distinguished past-president?—it is best to see what Darwin himself said.

Apart from a large number of brilliant deductions about the way worms live, Darwin proved that they eat raw and half-decayed organic matter and also pass through their bodies considerable quantities of earth. In this intermingling process they produce a rich vegetable mold or well-humified soil, and this is constantly being added to the upper surface of soils. To quote the original monograph: "Worms have played a more important part in the history of the world than most persons would at first suppose. In almost all humid countries they are extraordinarily numerous, and for their size possess great muscular power. In many parts of England a weight of more than ten tons of dry earth annually passes through their bodies and is brought to the surface of each acre of land, so that the whole superficial bed of vegetable mold passes through their bodies in the course of every few years. . . ." And again: "Worms prepare the ground in an excellent manner for the growth of fibrous-rooted plants and for seedlings of all kinds. They periodically expose the mold to the air, and sift it so that no stones larger than they can swallow are left in it. They mingle the whole together, like a gardener who prepares fine soil for his choicest plants. In this state it is well fitted to retain moisture and to absorb all soluble substances, as well as for the process of nitrification. . . ."

As the figure of ten tons per year per acre may seem surprising, it might be as well to summarize the evidence upon which Darwin based this estimate. He was led to believe that the weight of soil normally brought to the surface by worms was fairly high from studying the rate at which large objects such as big stones or even old ruins were gradually buried in the land. He himself and one or two interested friends collected and weighed all the

worm castings over timed periods on measured areas of land, on very small plots of about one square yard or so. If the areas were indeed rather tiny, on the other hand the time period was long, but in any case the run of various results was reasonably consistent. Working the figures out in enlarged terms of tons per acre per year, these cases are recorded by Darwin:

Place	*Plot Size*	*Period of Experiment*	*Equivalent Weight per Acre per Year*
Nice	1 square foot	About a year	14.58 tons
Chalk Valley	1 square yard	40 days	18.12 tons
Leith Hill (an old garden)	1 square yard	369 days	7.56 tons
Leith Hill Common	1 square yard	367 days	16.1 tons

Darwin was able to check the reliability of these figures by approaching the same problem in a different way. The above figures, if interpreted in terms of the thickness of the surface layer added to the soil assuming that the castings were evenly spread, lead to a layer about 1 inch to 1½ inches thick added each 10 years. In some quite different cases Darwin knew how long certain objects had been standing exposed on the land, and he was able to measure the *rise* of the soil surface around or over them. In five cases these figures were obtained:

Period *years*	*Equivalent Ten-Year Rise* *inches*
14.75	2.2
21.25	1.9
7	2.1
29	2.2
30 (poor land)	0.83

Now if there is not an accurate similarity in all these figures there is at any rate a very acceptable agreement of *trend* or *rough size,* and it seems impossible to quibble over Darwin's round average figure of 10 tons per year per acre, or a 1 to 1½ inches surface layer of *mold* each 10 years.

At this stage the modern tie-up can begin. An American measurement, quoted by Sir Albert Howard, shows that the soil of the castings is very much richer than the corresponding soil, thus:

	Pounds per Million of Soil		*Ratio of Castings to Soil*
	casts	*top 6 inches of soil*	
Nitrate nitrogen	22	4.5	4.89
Available phosphate	150	20.8	7.21
Active potassium	357.8	31.9	11.22
Humus	89,500	57,800	1.55

The point that Darwin made verbally in 1881 is thus well and truly confirmed in these 1942 figures from Connecticut Experiment Station. There may have been other similar measurements in the interim but, if so, little attention has been paid to them. 1881 to 1942 is a long time, and the humus school can well claim in this matter that *official* research has largely ignored a known biological factor in soil fertility.

With this point behind them the humus school then launches a strong attack at chemical fertilizers on the grounds that these materials discourage earthworms, drive them away and thus greatly diminish their powerful contributions. Where fertilizers are used the earthworm populations are low; additional supplies of chemical NPK are then needed to make up for the supplies from the soil's store that would otherwise have been made available by the worms; that, in short, is the argument. In particular the attack is directed at sulfate of ammonia.

In his preface to the republished Darwin book, Howard makes use of a rather unfair argument to establish his case that worms are driven away by fertilizers. He quotes American evidence from a United States Department of Agriculture Bulletin that earthworms are eliminated after regular application of sulfate of ammonia over a few years; and he also points out that this fertilizer is commonly used in the States for sports-ground grassland to reduce the trouble of worm-casts. Now these facts will not be disputed. It is the fact left out of the argument that makes

it unfair, or, at any rate, biased. Sulfate of ammonia is known to be an acid-inducing fertilizer; its regular use will inevitably reduce the soil pH to a dangerous level. However, it is the standard practice when using sulfate of ammonia to correct this with frequent liming, and where this is done the problem does not arise. Let no one think that this is a last-minute attempt to rescue a prisoner in the dock with belated evidence—the necessity for extra liming has been known for many years. It is surely not an admissible argument to claim that, because on golf-club greens and fairways sulfate of ammonia is deliberately used *without liming* in order to reduce the worm population and the cast nuisance, then also in farming practice, arable or pasture, the same effect will occur, for in these latter cases adequate liming should be taking place—if not, then we merely have cases of fertilizer misuse.

Now this, of course, is only a verbal argument, a defense in terms of ideas, but which can be well supported by experimental data. It is perhaps not widely known, but—to their infinite credit—the golf clubs of Britain maintain a research station of their own at St. Ives, Bingley, Yorks. There a small staff of scientists study the cultivation problems of fairways and putting greens. I believe it is true to say that some work is done also on bowling greens and that during the war a great deal has been done to solve aerodrome grass problems. The golfer who misses a putt or gets diverted into the rough because of a worm-cast is unlikely to share Charles Darwin's views upon the value of the earthworm. And so, paradoxically, we find that careful numerical work on the effects of various fertilizers upon worms has been carried out at St. Ives, at a research station that has to consider worms as enemies of society. A nice example of the law that one has to become a nuisance to be much taken note of.

At St. Ives they maintain a large number of comparable grass plots undergoing specific treatments of all kinds, chemical, organic, and mechanical. The Director, R. B. Dawson, M.Sc., in his book *Practical Lawn Craft* (Crosby Lockwood), gives these figures for plots to which the same amount of nitrogen is regularly

applied as sulfate of ammonia without complementary liming and as nitrochalk:

Sulfate of Ammonia (no lime)	*Nitrochalk*
nil	15.4 casts per square yard

These plots, by the way, started off by being worm-free so that the above count of casts represents the extent of re-invasion by worms.

This comparison shows that chemical nitrogen applied in a non-acid form, for nitrochalk contains 48 per cent calcium carbonate, encourages worms, and by contrast that the acid-tendency of sulfate of ammonia severely discourages worms. It is tempting to assume without any further checking that the factor affecting worms is solely and sheerly acidity. We must not risk deductions of this kind, plausible though they may seem. We have no right to feel certain that sulfate of ammonia will not go on keeping worms away even if lime is added. At St. Ives plots were also maintained where regular liming accompanied the sulfate of ammonia treatment, and in these cases this figure was obtained:

Sulfate of Ammonia with Regular Liming
30 casts per square yard

I do not think it is necessary to drive home the argument of this last result with further comments, but, as additional evidence that it is soil acidity that discourages worms, another St. Ives comparison might be quoted. Two plots were treated with sand, one with a lime-free inland sand, the other with a sea-sand containing about 4 per cent lime. After 4 years, both plots having been initially worm-free, the cast-counts per square yard were as follows:

Lime-Free Sand	*Lime-Containing Sand*
4.6 casts per square yard	8.0 casts per square yard

It is quite reasonable, therefore, to conclude that worms are not discouraged where chemical fertilizers are used so long as

such treatments do not set up soil acidity that is uncorrected by liming. Since in any case such acidity should be neutralized by liming because of its effects upon the beneficial microorganisms, it is equally reasonable to conclude that the proper use of fertilizers will not be attended by reductions in earthworm population or activity. In a recent joint paper in the *Scottish Journal of Agriculture* Dr. W. G. Ogg, the present Director at Rothamsted, and Dr. Hugh Nicol have stated that Rothamsted experience shows no earthworm reduction even on soils that have received abnormally heavy fertilizer applications, *provided that* the soil has not been allowed to become too acid.

This, then, is the answer to the humus school's charge that fertilizers drive away worms. Nevertheless, it should not be taken to mean that earthworms are not encouraged by humus manuring. That, indeed, is fairly well established in Darwin's original work, organic matter raw or part-decayed being the very food of the worms. Some Rothamsted figures quoted by F. H. Billington in his book, *Compost,* confirm this view. Population estimates were as follows:

Unmanured land	0.50 millions
F.Y.M. manured land	2.75 millions
Grassland	8.60 millions

Thus once again it seems that some powerful pro-humus evidence that can be freely accepted by even the most enthusiastic fertilizer advocate has been unjustifiably expanded into anti-chemical evidence.

However, it may still be argued, what about the 10 tons per year per acre, the castings that have been proved to be so much richer in active NPK and in humus? With such a contribution are fertilizers so necessary? Surely this contribution has always been present whether or not fertilizers have been used—except, of course, where acidity has been wrongly permitted to arise. The earthworm probably provides one of the major ways in which the soil's inactive NPK is turned into active NPK, but, as this book has already tried to show, the total natural rate

of this kind of change is not enough for our cropping needs without extra help. In addition, it must be remembered that Darwin's figures were obtained from grassland observations, not from cases of arable land. A glance at the Rothamsted figures just given shows that over three times as many worms inhabit grassland as inhabit even arable land that is regularly manured. This certainly suggests that the earthworm's contribution to arable land might be stepped up by *very heavy* humus manuring, and this might reduce the need for fertilizer application, but, to bring in another point that this book has tried to establish, we have already seen the enormous amounts of manures that such a universal policy would require are not likely to be available.

Nevertheless, it seems clear that Darwin's work should be followed up on a wide scale. The earthworm function in fertility is considerable and it has been given little enough attention, especially in arable cultivations. It might be that large-scale survey investigations could lead to a working figure for the optimum contribution from worms in various soils; a proper population per acre for this contribution could be assessed, and fields which showed lesser populations might be made more naturally fertile by both humus treatment and by worm inoculation. This kind of work is actually being done in America on a commercial scale. A company, started by a doctor who enterprisingly followed up the Darwin work, sells earthworm eggs in capsules, in gallon cans of cultures, and in what are called spawn bricks. Remarkably good results are claimed in this organization's sales-literature. Still, no one would doubt that earthworm encouragement in very poor soil would show noticeable improvements. Customers would not be likely to pay a dollar and a half for 100 capsules (equal to 300 eventual worms and said to be the dressing for two window-boxes or for one tree or for a six-feet by eight-feet flowerbed) if they had not first felt that their soil was pretty bad.

Summing up, then, three specific charges against fertilizers have been considered. There is no antichemical evidence that stands analysis in regard to the depression of mycorrhizal fungi,

and, even so, there is not yet a fully established case that these mycorrhizal organisms are generally vital to fertility or to plant-growth. There is no antichemical evidence that fertilizers inhibit the beneficial bacteria of the soil, except that the azotobacter may be reduced in activity in the presence of plenty of active nitrogen, but even this is only a plausible speculation. There is not only no evidence that fertilizers drive away earthworms, there is clear and numerical evidence that they do not do so when properly used.

However, all this concerns only these three detailed ways in which fertilizers are said to damage the soil's natural arrangements. Would details matter if the general overriding accusation was true? If fertilizers do indeed produce crops of poorer stamina and health and food of lower nutritional value, then the reasons for this would be only of minor and academic importance. Next the evidence for this general indictment must be considered.

CHAPTER XIII

HUMUS AND HEALTH

"The Law of England presumes every man to be innocent until his guilt is established, and it allows his guilt to be established only by evidence directly connected with the charge against him." LORD CAMPBELL, Chief Justice, summing up in the trial of Palmer, the poisoner, 1856.

WE MUST be logical in the search for truth. We must analyze the statements made by the humus school about plant-health; and then classify them as to whether they say:

1. That plant health follows humus applications, or
2. That plant ill health follows chemical applications.

If the humus enthusiasts forget to make this vital distinction, it must be made for them. Certainly it is bigoted to the last degree to assume that the second statement is *ipso facto* proved by the proof of the first.

The charge that chemical fertilizers are a prime cause of unhealthy growth is shown by the following quotations:

"Diseases are on the increase. With the spread of artificials and the exhaustion of the original supplies of humus carried by every fertile soil, there has been a corresponding increase in the diseases of crops and animals which feed upon them." Sir Albert Howard, *An Agricultural Testament.*

"My canes (raspberry) have not had any chemical fertilizers, and in consequence have not required spraying. In this, as in other cases, no chemicals means no sprays." F. C. King, article in *The Market Grower,* March 18, 1944.

"The accelerated growth induced by chemical fertilizers has

the effect, among others, of speeding up the rate at which humus is exhausted. As this depletion of humus proceeded, troubles began. Parasites and diseases appeared in the crops, and epidemics became rifle among our livestock, so that poison sprays and sera had to be introduced to control these conditions." E. B. Balfour, *The Living Soil.*

"Now sulfate of ammonia and many other artificial manures are likely to kill the earthworm and bacterial life of the soil, and so one gets ill-nourished plants which are liable to fatal attack by disease and insect pests. Disease, fungus, and insect pests are always with us, but they chiefly affect the unhealthy plant." Lord Lymington, *Famine in England.*

It is only too obvious that if these opinions are true, then all the evidence for fertilizers which we have seen in Part One of this book must be untrue, or at any rate misleadingly futile. It is not very useful to gain a season or so of extra cropping with chemicals only to find that epidemics of disease and pest attack eventually sweep through the farm.

The mere statement of these opinions does not establish them. Certainly they are stated with a degree of assertion and assurance that would be called dogmatic were it to accompany the opinions of orthodox science. It is no matter of verbal dueling or of speculative debate in plausible theories; it is a matter of practical data and facts, of investigation not only of isolated individual cases but of general experiences. Great care must accompany every deduction from any mass of evidence for such deductions are being made from data which have been influenced by a large number of variable factors. And in all this analyzing, in all this digesting search for truth, it must be remembered that we require not evidence that is only pro-humus but evidence that is directly antichemical.

No one has presented the antichemical general indictment more vigorously than Sir Albert Howard. To quote a few lines from his major work, *An Agricultural Testament:* ". . . No attempt has been made to disguise the conclusions reached or to

express them in the language of diplomacy. On the contrary, they have been stated with the utmost frankness. It is hoped they will be discussed with the same freedom and that they will open up new lines of thought and eventually lead to effective action."

Howard's book is, therefore, exciting. He attacks with all the gusto of passionately held convictions. And in one sense this very readability is apt to be misleading. By comparison the dull and plodding treatises and textbooks of orthodox science make a much humbler impression upon the mind and memory of the average reader. Howard's hard-hitting and sweeping denunciations and his reformer's rhetoric are colorful and compelling.

By this I do not for one moment suggest that Howard's book should not be read. On the contrary, it would be far better if readers now put a marker in my book and reopen it only after a session with Sir Albert's Testament. What I do suggest, however, is that Howard's thesis should be read with dissecting concentration so that the bones of the arguments are always filleted out of the lively flesh of spirited writing in which they are encased. For the tests must be: (1) is this proposition based upon reliable and sufficient evidence, and (2) are the further deductions from that evidence so logical that there can be no other deductions?

Many will prefer Lady Eve Balfour's *The Living Soil,* a more recent effort to cover much the same ground and undeniably an agricultural *best-seller.* Lady Eve is just as convinced as Howard that chemicals are wrong, but she tackles the presentation in a gentler manner. She does try to separate evidence which she considers to be directed and positive from evidence which is only circumstantial and indicative. Her theme is that the case against chemicals is so powerfully indicated that research must forthwith get down to the job of settling the issue one way or the other. This is in sharp contrast to Sir Albert who regards the case against chemicals as settled and proved and whose demand is not so much for a testing of the case as for the immediate abandonment of the fertilizers. I do not think I am being at all sweeping in saying that the chemical indictment is presented so adequately

and skilfully in these two books that the other books from the same school, though often interesting, are less significant.

When a chemist and an orthodox believer attempts to summarize the Howard-Balfour thesis, he is apt to find it melting away in the process. In a recent speech in the House of Lords when soil fertility was debated, Lord Hankey expressed the cause of this in these words:

"There is more common ground to begin with in this matter than is generally realized . . . there is common ground as to the great importance of humus in the soil. There is common ground also that, whether you have artificials or not, you must have an adequate supply of organic fertilizers. Again, compost is admitted by the supporters of chemicals to be a very valuable form of organic fertilizer. . . ."

That is my difficulty now—for so much of the Howard-Balfour argument is based upon this *common-ground* evidence.

Howard supports most of his argument upon two main planks and a subsidiary theory.

1. Comparison of Eastern and Western agricultural practices to the detriment of the latter.

2. Successes where his Indore compost has been used.

3. The mycorrhizal theory as a major explanation of the successes with compost.

Asiatic growers, says Howard, have maintained their soil fertility and crop-health through 40 centuries without chemicals and simply by returning all organic wastes to the soil as humus-forming manures. They do not suffer from many of the troubles of our much younger farming; they have never fallen back in the task of humus maintenance. This then should be our Western method too.

Here a comment on the logic of the argument must be interposed. Even the most perfect health of Eastern crops (incidentally a fact which many authorities could never accept) could not be an argument *against something that Eastern growers do not use.* It is only an argument for what they do use—humus manures. A man may happen to keep very fit on a strict diet of fish. That

is good evidence for fish. However, it is obviously not an argument against meat or poultry or other foods than fish. The Eastern growers do not use chemical fertilizers; they go to very great trouble to obtain all the organic wastes they can and they accept whatever cropping-level and standard of nutritional living this policy provides. I am not denying that this might be successful; I am only pointing out that all its success is *only pro-humus evidence,* that is to say, common ground to both the humus school and to orthodox science. No admissible evidence about chemical fertilizers and their effects can logically come out of the Eastern world except where Eastern growers use fertilizers. And such evidence would not be antichemical unless unhealthier cropping resulted. I know in advance what the humus enthusiasts' reply to this will be—they will say that the evidence comes out of comparison, comparison of the all-healthy Eastern humus-raised crops with the troubles of Western crops where chemicals are used. Quite apart from the correctness of the facts of this comparison (of which I am most doubtful but which I cannot check from any personal Eastern experiences) can anyone accept this kind of comparison in which there are so many other overriding variables than manurial and fertilizer policy? What of the climate differences, the soil differences, the crop differences, and so on? It is magnificent, but it is not logic.

To return to the Howard thesis. Attributing the successes of the East to this humus effort, Sir Albert devised his now famous Indore composting system, basing this upon a study of the natural production of the rich humus soil of forests. With this type of compost he was able to achieve a number of personal successes on the Indian plantations of which he was in charge. A large number of varied kinds of growers subsequently sought his advice, and this advice invariably was to tell them to adopt composting and the humus-only policy. In his book he reports a convincing list of successes—coffee, tea, sugar-cane, sisal, maize, rice, etc., and in this country vegetable farming and hops.

Thus from the management of an English farm adopting the Howard ideas, here is a memorandum extract:

"The farmland is not yet independent of the purchase of fertilizers, but the amount used has been steadily reduced from 106 tons in 1932 . . . to 40½ tons in the current year. . . . The potato crop which formerly was sprayed four or five times is now only sprayed once, and this it is hoped will also be dispensed with before many years when the land has become healthy and is in a proper state of fertility. . . ."

Almost all Howard's evidence about these successes is qualitative rather than quantitative. This makes it difficult to analyze, but it is not surprising for the humus school as a whole has rejected the numerical expression of soil fertility factors and changes. They attack the orthodox research efforts in this matter of measurement by saying that unknown items in the balance sheets are always left out so that the balance sheets are meaningless. Thus Howard: "Mother Earth does not keep a passbook. Almost every operation in agriculture adds or subtracts an unknown quantity to or from the capital of the soil—fertility—another unknown quantity." This mystical attitude to soil biodynamics may indeed have something to be said for it—it would be dogmatic to pretend that agriculture is merely a branch of applied mathematics. But there are many things which *can* be measured even if the resultant figures only represent a 90 per cent estimate of the facts, and where such measurements are possible we have every right to make use of them in the pursuit of truth even if truth herself in the end be immeasurable. Granted, it can be replied that quality is more important than mere quantity, but science can measure quality too, at least in many of its aspects. And it seems difficult to believe that the very complex dynamic changes represented by the growth of a plant, occur yet with lower quality. In such an intricate series of bio-chemical change, one might be justified in thinking that, if the quality-producing changes were affected adversely, the same would happen also to the size-controlling changes. I find it difficult to believe that an operation which results in a much bigger turnip should result, however, in one that is much less healthy, less nutritious.

Nevertheless, without a quantitative approach, having rejected this method of analysis, Howard prefers his practical successes to all the measured plot-test evidence of orthodox agricultural research. Small plots, he claims, "cannot represent the agricultural problem they set out to investigate." He objects also to the entry of higher mathematics to scrutinize a *torrent of field results*. Yet mathematics, even higher mathematics, is a science of rigid honesty, and its use in the interpretation of results is common to many branches of research. The actual effect of the entry of the statistical scientist into places like Rothamsted has tended to trim the deductions from tests rather than to act as a kind of camouflage for heterogeneous results.

Here then we have a case built up upon practical evidence of this general nature: ailing crops with fertilizer treatment—change to compost-only policy—crops become healthy. The deduction is that chemicals must be abandoned, that all agriculture must make the same change. Is it possible that in this clash there is really no clash of evidence at all, but only the clash of deductions, of interpretation? We cannot reject Howard's records of successes even if we might wish that they had been reported with a shade more emphasis upon figures. Nor can we, as Howard does, sweepingly reject all the perhaps over-numerical evidence of orthodox plot-tests. Here are honest observers presenting reports from which they deduce totally opposed ideas.

To resolve this dilemma, the general successes of the compost policy must be analyzed from a different standpoint than the pre-experimental conviction of the humus school. For they began with a passionate idea, which is a dangerous thing to do in trying to uncover truth.

The first point to be made about the compost-school's successes is that a high proportion have occurred in hot, dry countries; at any rate in countries where there is more heat and there are longer drought periods than in our own climate. Now surely there can be few soil conditions that need humus more essentially than where hot droughts are regularly experienced and where rain falls in ill-spaced immoderation. The importance of

colloidal humus as both moisture conserver and soil binder will be much greater in such climates than in our own more regular mixture of all weathers. It does not seem to need the mycorrhizal theory to prove that humus is indispensable for such soils. Second, these various plantations would not have sought help had their previous cropping been satisfactory—successful farmers and growers do not call in outside experts any more readily than fit people send for doctors. Or, even if this is not admitted, it is hardly likely that the somewhat cumbersome compost system would have been embarked upon had all been well before.

It seems, therefore, not unreasonable to suppose:

1. That previous troubles were caused by over-reliance upon chemical fertilizers and/or by underattention to the complementary need for humus.

2. That lack of humus had thereby become the main limiting factor in cropping both for health and quantity of crop.

3. That the abrupt change to heavy humus-manuring corrected this deficiency, a correction that could not have been made by the continued policy of NPK chemicals.

Under drought-liable conditions, the effect of humus shortage is likely to show itself more quickly, and similarly it may be supposed that the correction would also show beneficial effects more quickly. Thus, the orthodox view of soil fertility—that humus and NPK supplies in chemical form are complementary and not opposed—can adequately interpret both the Rothamsted type of evidence and the humus school's evidence. But the humus school's idea can interpret only its own evidence, and must attack and discount the research station data whenever this stands opposed to the humus-only theory.

Sir Frederick Keeble, late Director of the Jealott's Hill Research Station, argued on not dissimilar lines in his book, *Science Lends a Hand in the Garden*. He pointed out that results of humus application to Indian soils may well apply only to Indian soils and not be paralleled on richer European land. Indian yields of cereal crops common to both agricultures are often not much more than a third of European crop yields; Indian soils

are poorer in nitrogen and even poorer still in carbon. The general lack of carbon—judged as a measure of humus—may well be the major limiting factor to good and healthy cropping. Hence the success of any manurial policy such as that of the Indore composting scheme where heavy applications of carbonaceous matter are provided.

A further point about most of these overseas successes with compost is that, having achieved success, the growers have every practical reason to continue with the policy. They enjoy the necessary twin conditions for economically successful composting—very cheap labor and abundant waste organic materials. The internal combustion engine has not so universally invaded their roads and towns, the sanitation engineer does not remove all the human animals' wastages, the local standard of living has not created an ever-rising schedule for farmworkers' rates. Who, under such conditions, would *not* pursue a policy of composting all he can? Especially when the cost of manufactured fertilizers from industrial centers may well be relatively high through transport costs.

Here the main trend of the discussion must be interrupted to analyze a plot-test quoted by Sir Albert Howard, a comparative test which is claimed to show the *badness* of chemicals and the *goodness* of humus manures.

Tea-seedlings were grown on subsoil plots. Subsoil was deliberately chosen so that the influence of any humus in the normal soil was ruled out. One plot was manured with compost at the rate of 20 tons per acre; the other plot with the equivalent NPK supply as chemicals. After 9 months, the humus plot seedlings were *by far the better,* 10 inches high, branched, with dark-green leaves. On the chemical plot seedlings were only 6 inches high, unbranched, with sparse pale and unhealthy foilage. From this test Howard draws the deduction that compost produces healthy plants and chemical fertilizers do not.

The scales of justice could hardly have been weighted more heavily against chemicals than by this decision to carry out the test upon subsoil. It would be almost as reasonable to design a

test with one plot treated with NPK nutrients, but with no water, and the other plot supplied with water only—and to deduce from the obvious results that water was better than chemicals. That the tea seedlings on the chemical plot survived at all is remarkable; and, with so high an application of compost as a rate of 20 tons to the acre, so obviously sufficient to cover *all* needs, the excellent health of those seedlings is hardly surprising. This test is an admirable example of the correctness of orthodox scientific opinion. It illustrates that chemicals need a complement of humus, and it illustrates that humus manures can be all-sufficient *if enough can be applied* to cover both humus and NPK needs. When Howard deduces that chemicals alone are inadequate, he is 100 per cent right; when he extends this to the deduction that chemicals should not be used at all, he is fallaciously expanding the test to represent conditions which it did not cover. What fertilizers do in absence of humus is no criterion as to what they will do when humus is present.

Had this attitude been expressed only by Howard, it might have been regarded as one man's view and not the view of the humus school collectively. Lady Eve Balfour too has insisted upon the need for subsoil tests to measure chemical effects. Thus she has said:

"The only short-term test therefore which could possibly provide a true comparison would be one undertaken with subsoil, in the one case with inorganic nutrients added, and in the other with the addition of humus."

Resuming the analysis of the humus school's successes, we cannot argue that drought conditions give humus so high a priority in their cases nearer home. On the other hand, there are much greater obstacles to the adoption of the all-out composting policy—organic wastes are less to be had for the asking and labor is much dearer than native labor. We can certainly feel sure that no farm or estate in an industrialized country would adopt such a revolutionary change unless previous results were so bad that this consideration outweighed all others. It can be supposed, then, that previous severe troubles existed, and these may well

have been due to over-reliance upon NPK chemicals and under-attention to humus. It is impossible to be cocksure about this for the case-histories are not presented with data as to soil analyses, etc., though the NPK contents and organic matter contents or even carbon contents would have been illuminating, certainly at least as useful as a small flashlight in a total black-out. If the limiting factor to good cropping and plant health had been humus, then NPK fertilizer applications would not so much have been taken up by the plants as left in the soil or leached from the soil.

Then comes the abrupt change to the compost policy. It is only too certain to be successful. The limiting deficiency of humus ceases to operate, and the store of phosphorus and potassium if not nitrogen from past ineffective fertilizer practice is probably able to supplement any nutrient needs above those provided with the compost. Is this special pleading? If so, then the humus school must be reminded that they frequently argue that fertilizer successes rest upon the store of humus in the soil from past manurial policy, and all I am doing is to present the converse argument.

In 1932, 106 tons of fertilizers were used. According to information (in the report given by Howard but not requoted in the extract above) the acreage was 225 arable, 35 horticultural, and some 70 of permanent or rough grazing grass. Now this 106 tons was no small intake in comparison with the average use of fertilizers in British farming of 1932. After the adoption of the compost policy, this drops not to nil but to 40½ tons, and this is still quite a fair average rate of application, especially if we reasonably assume that most went on to the arable acreage. I am quite sure that no fertilizer firm would be dissatisfied with the amount of their business for the acreage concerned if their annual order was 40½ tons. There is again nothing in this case which is not acceptable to the orthodox school in facts for interpretation.

Quite apart from these cases quoted by the leaders of the humus crusade, cases of similar nature are frequently quoted by

market-gardeners in their journals. One article of this kind recently attacked fertilizers with some severity, but in the course of the article the writer revealed that he made annually about 200 tons of compost manure which he applied to something rather less than ten acres of intensively cultivated land. Because he obtained good results by this manurial policy, he argued that fertilizers were unnecessary. This is, of course, not evidence against fertilizers at all—it is merely evidence for the orthodox opinion that you can supply all plant needs with manures so long as you can get enough. Suppose all growers adopted the same 200-tons-per-10-acres compost policy? Reference to chapter seven will show where this argument leads—into the never-never land. This compost enthusiast was expediently building up his own fertility with the composted waste fertility from many more acres than his own. Millionaires might, with equal justice, regard poverty from the comfort of the Savoy grillroom and wonder why everybody was not content with the economic *status quo*.

It seems time to bring this part of the defense of fertilizers to a conclusion. It is, I suggest, a clear-cut conclusion—that a great deal of the humus school's evidence which is presented as antichemical is in reality fully capable of orthodox interpretation as pro-humus evidence and nothing more. For nobody is quarreling with the Howard school about their insistence upon the essential place of humus in all fertility maintaining operations. Their opinion that these cases are also examples of antichemical evidence is based upon a misconception of correct fertilizer practice, which includes side-by-side attention to the humus needs of soil.

Unhappily this misconception acts as a barrier to useful collaboration between the two schools of thought. For example, the humus people have their own research center, the Haughley Research Trust, in East Suffolk. Lady Eve Balfour is actively concerned with the work there as farm manager. She is directing such limited tests as are now being carried out, but when the full plan for large-scale research commences other experts will also be sharing in the direction. Inasmuch as Lady Eve's own plan for research—as explained in her book—is likely to be the

basis of the eventual program (and after all her book has quite clearly shown her to be the most logical leader of this school of thought), inasmuch as this may be the case, the total research would seem to be committed to a major comparison of large plots cropped on fertilizers only and similar plots cropped on compost manures only. That is to say, a large amount of work and a great deal of thought both of the highest order, backed by private funds and untied to official permissions or guidances (this last a great asset!) may be applied to solve a problem that is largely, if not wholly, already settled. For we know already that the continued use of chemicals without humus provision usually only proves that chemicals by themselves are not efficient builders or maintainers of fertility.

In an article in *The Fertilizer Journal,* written after the publication of her book, Lady Eve revealed a change in policy—the inclusion of a treatment in which chemicals and manures are used together, but it was still firmly held that the main issue was to be this comparison of the chemicals-only and compost-only series. It seems a tragic loss of a notable opportunity. It is, however, under consideration that humus may be given to the chemicals-only plots provided that it is compost made with the use of a chemical nitrogen-supplier and not with an organic type like dried blood. This surely must not remain merely under consideration but *must* be included as a vital part of the test-planning. For, with its inclusion, the two schools have at least met halfway, and the basic misconception seems considerably removed.

What this research station could establish, and no better organization would seem to exist in view of its expert practical knowledge of composting, is the minimum amount of humus manures needed to complement chemicals. Thus treatments could be devised on something like these lines:

A. NPK fixed quota plus X quantity of compost.

B. Same NPK quota plus 2X quantity of compost.

C. Same NPK quota plus 3X quantity of compost.

The proportions could be more varied than this, but we should

then be approaching a possible solution to a problem that seems so far untackled. However, I fear that the humus school cannot constitutionally admit that such a problem really exists. However, I do not suggest the program to imply that humus is like an important man's unpopular wife who has to be regretfully invited to parties with him; I suggest it because there is a lower limit to humus supplies in industrialized countries while there is relatively almost no limit to the chemical supplies, and therefore the safe minimum complement of humus would be a highly important value to estimate.

However, we have more to discuss about the antichemical case yet. There is still the specific accusation that chemicals induce disease; and after that the accusation that they produce foods which are less nourishing. We have not done enough yet to vindicate the claims and results of Part One.

CHAPTER XIV

PLANT DISEASE

"All that can be said is that probably three factors cooperate in determining whether a plant shall keep healthy or become diseased. They are constitution, cultivation, and chance. Hardy says in one of his novels that propinquity is the chief agent in friendship. It certainly is of disease. A sick plant growing in the next garden may cause disease to ravage our own plantations." SIR FREDERICK KEEBLE, *Science Lends a Hand in the Garden.*

THE PROSECUTION states that plants raised with chemicals are less robust, less able to withstand the attacks of fungi, pests, and viruses; so that epidemic ill health results. This being so, extra yields are short-term and illusory benefits, quantity and not quality, and quantity in any case that must be frequently discounted by severe loss.

The humus school have suggested why this happens, and we have already analyzed their evidence for specific charges against chemicals in chapter twelve. Their principal explanation, the mycorrhizal nutrition theory, as we have seen, is by no means established. However, details hardly matter—a fact is still a fact whether it can be explained or not. And we should be able to decide whether the use of fertilizers has increased diseases and attacks by pests—it is the kind of thing that can be assessed reasonably well by observation and measurement; in the widest sense, indeed, by mass observation and statistics. We can therefore approach this issue from quite a different standpoint from that of the last two chapters.

To support the charge that chemicals actually create or increase ill health there is little clear-cut evidence. Take away the evidence that is only pro-humus, the kind we examined in the

last chapter; take away extreme cases due to the obvious misuse or overuse of inorganics, and not very much remains. It is an interesting analysis to read the principal indictments of the humus school and to see just what *direct* evidence supporting this charge there really it. It is true that fertilizers are blamed for many ailments, but the reasoning upon which the blame rests is of the kind that ignores all other possible culprits. Sweeping deductions are piled upon sweeping generalizations and both are open to many questions. Yet this is hardly surprising. For, in over a 'century of measured fertilizer research, carried out at many centers and often by great scientists, increased tendencies to disease and death would hardly have passed unnoticed and unrecorded. Indeed, what has been recorded by orthodox science points to some extent in quite the opposite direction.

These connections between NPK chemicals and plant health have been established:

(a) NPK insufficiency produces weakly, unhealthy plants, which quickly succumb to pest or other kinds of attack.

(b) The rapid *green* growth arising from the over-application or from the unbalanced application of chemical nitrogen is more liable to fungus attack.

(c) Potash applications very often minimize plant ailments by increasing the plant's capacity to resist attack; conversely, potash deficiency is a favorable factor for many plant diseases, e.g., chocolate spot of beans.

These statements are based upon a large number of careful investigations on both small and large scale. That there probably have been a number of ailments induced by the misuse of nitrogen is undeniable. There are each month far too many deaths upon the roads due to the misuse of the internal combustion engine—the remedy is not to do away with the motor, but to see that much more is done to secure its proper and reasonable use. To repeat a point that has already been made, or rather a double point, it must be remembered that fertilizer misuse is often caused (a) by wartime supply troubles, and (b) by peacetime economic difficulties. Science can hardly be blamed

for this; at any rate not only scientists, but everyone else as well must go into the dock so far as these deeper-rooted world maladjustments are concerned.

Now if this accusation that NPK chemicals cause disease is to be accepted, two primary points must be established:

1. That plant disease *has* increased since the general introduction of fertilizers.

2. That, if this is so, it is the use of fertilizers that has caused the increase in disease and *not* any other factors that have also changed.

Let each of these points be considered in turn; and, even if the former point cannot be established, let the second point still be examined even though it really depends for its existence upon the proof of point one. By such an analysis of facts, we should be able to decide whether the accusation is valid or false.

It is difficult to make assertions about the amount of plant disease that exists at any time, still more to compare one period with another. Plants are not panel patients and plant growers do not always call in plant doctors—statistics, therefore, are not freely available, and we have to rely upon *general impressions*. Furthermore, the amount of disease in any period must be related to the amount of cropping; just as losses of planes in air attacks must be related to the number of bombers and fighters sent out. We have one very useful criterion. Our farming before the days of general fertilizer use was based upon an all-humus (plus lime) policy. Therefore, in a broad sense, plant disease epidemics should have been less severe than those we now experience if this charge against fertilizers is true. The absence of reliable statistics does not entirely invalidate this kind of comparison for when we are dealing with major outbreaks of disease, epidemic outbreaks, the general impression type of record is likely to be of reasonable value.

That there always has been some disease and that there always will be is indisputable. How much used there to be, and how much is there today? I think we are prone to exaggerate the modern quota and at the same time to believe that things were

much better in *the good old days*. Psychologically we love this kind of simultaneous self-depreciation and ancestor-worship. We forget that these old-time glories were due only to the fact that our forefathers got in on the non-competitive ground floor of that same industrialism whose uncontrolled competition is now doing its best to wipe us out.

Why is it easier to exaggerate the amount of disease today? To begin with, when a crop today shows signs of trouble, the grower is more likely to make a fuss about it, a very right and proper fuss. More is known about diseases and there are more expert plant pathologists and advisory centers to give advice. Diseases can be diagnosed and named with far more accuracy, and there are plenty of remedies in many cases. All these factors naturally cause more *talk* about disease, more counter-action. There is the same psychological tendency in discussions about human health. There is more cancer, more gastric breakdown; more tuberculosis, and so on—these conversational assertions are made freely. However, how can anybody compare the scale of things today with the scale of a largely unparticularized century or half-century ago? Then, people just died, died from these same specific afflictions without accurate diagnosis of the thing that killed them. Today Mr. X dies from cancer and all the neighbors can discuss it until the next local funeral, but Mr. X's great-grandfather, who died from the same thing, simply dropped out with colic or premature old age or whatever else the puzzled G.P. like to suggest. Greater knowledge about troubles inevitably creates a general impression of greater affliction. If anybody doubts this, let him try reading a medical textbook filled with descriptions of symptoms. Jerome K. Jerome drew attention to the effect of this procedure many years ago, and pointed out the necessity for following the course of study with a large rump-steak.

Nevertheless, there does happen to be quite a fair amount of evidence about what did happen before the days of NPK and it seems that plant diseases did take grievously severe toll of our crops. Having agreed in the preceding chapter that humus ma-

nures are vital to plant-health, it may seem I am now having it the other way round since in those days humus-only was the rule. The terms of this argument and the raising of it have been determined by the humus school. Therefore, within those terms, it is logical to claim that the argument is unsound *if* it can be shown that some of the most severe epidemics of plant disease occurred before the era of chemical fertilizers.

One of the most valuable books about plant disease is a historical survey of plant troubles by E. C. Large, *The Advance of The Fungi*. Large is not only an eminent modern plant pathologist but also an occasional and distinguished novelist; consequently his book is a masterpiece of scientific and yet most readable statement and comment. It may seem irrelevant here to venture into praise of another book, but *The Advance of The Fungi* is far too little known, largely, I think, because it was published in 1940 when other survivals than those of plants were concerning most people and partly because it is rather a formidably sized book. However, no one interested in plant growth should fail to read it. Anyway I must acknowledge the greatest indebtedness to Large for many of the facts that follow.

After all, neither chemists nor experts in particular soil treatments should have the last word about the causes of plant disease. This belongs to those who have specialized in that subject—the plant pathologists. The humus school, if I judge their attitude correctly, will not concede this point. They are vigorously opposed to the departmentalism of modern science and research. They insist upon the approach via the study of health rather than via the study of ill health. This is, of course, no new argument; it has been used against the medical profession and its methods for many years. Howard and Lady Eve Balfour both use it in their books to criticize the agricultural research system in which certain workers specialize in the study of disease and disease symptoms and so on.

There is a great deal to be said for this argument. The reasons why Mrs. Jones keeps so remarkably fit while Mrs. Smith keeps so damnably unwell are important. To suggest that research

workers ignore this approach to their problems is factually unsound. In medicine, the doctor must inevitably spend most of his time examining the unwell and therefore his experience tends to become a knowledge of human derangements. However, the progress of medical science is largely in other hands, in the hands of research workers who do increasingly take into account the complementary and comparative study of health; the research worker in nutrition, for example, who is likely to be a biochemist and not a doctor, spends a great deal of time checking vitamin intakes of people who are reasonably fit in order to check intakes of people who are not. Similarly, the plant pathologist or mycologist at an agricultural research station is not such a fool as to ignore the conditions that have allowed one crop to thrive while another adjacent crop has been struck down; at any rate, if he is such a fool, then he is a very bad scientist. Of course, the agricultural adviser, though possibly based upon the same research station or some county organization, corresponds to the doctor and he must of necessity deal more often with disease than health. Both the doctor and the adviser have to deal with patients. It is too late to talk about health when health has gone. The patient who is ill hardly wants to know how he might have avoided becoming ill, he wants relief and cure because he has already lost the chance of avoiding whatever it is he has got. A farmer with a crop showing symptoms of disease first wants to know how to check the disease, not to be told how he might have avoided it. Though this information might help him in the future, it cannot help him at the immediate moment.

The picture of over-specialized, rule-of-thumb and red-taped research that is vividly painted in *Agricultural Testament* and clearly suggested in *The Living Soil* is not an accurate picture—it is a caricature. I can only recommend a thorough reading of Large's book to convince any skeptic that modern pathology is a study that considers both plant health and ill health.

In any case the evidence I am borrowing from Large's work is not so much a matter of opinion as of recorded fact. The pre-

ceding paragraphs have been made necessary only because the humus school has so frequently discredited the specialized plant pathologist as an admissible witness in the trial of fertilizers, and I have therefore had to prove that on the contrary the pathologist is a leading witness.

First, potato blight—one of the most serious diseases that plants are heir to, one that has caused millions of deaths from starvation, created far-reaching political crises and consequent political decisions that still powerfully affect our lives today. Some quotations both from Large and from records which he unearthed will tell us a great deal about potato blight.

"A fatal malady has broken out amongst the potato crop. On all sides we hear of the destruction. In Belgium the fields are said to have been completely desolated. There is hardly a sound sample in Covent Garden Market. . . ."

This is a quotation taken from the *Gardeners' Chronicle*, August 23, 1845. Large continues in his own words:

"As the weeks went on, into September, the reports of the spread of the disease, from Poland, Germany, Belgium, France, and from all over England except a few districts in the north, proved that Dr. Lindley's fears were not exaggerated. Every kind of potato was attacked. . . ."

From the *Gardeners' Chronicle* of September 13, the same year:

"We stop the Press, with very great regret, to announce that the Potato Murrain has unequivocally declared itself in Ireland. The crops about Dublin are perishing. . . ."

The effect of potato blight upon Ireland was devastating. Famine swept through a country that lived on its potato crops. Commissions were set up, botanists proposed theories, Peel proposed the repeal of the Corn Laws to bring in extra food. Said Peel:

"Good God, are you to sit in Cabinet and consider and calculate how much diarrhoea and bloody flux and dysentery a people can bear before it becomes necessary for you to provide them with food?"

And, while the debate went on, there were relief shipments of maize from America.

However, in the August of 1846, when the potato crops had at first looked like succeeding, the blight descended again. Once more the political crisis was acute, for Ireland must starve for a second winter. To quote Large:

"The potato blight fungus . . . had revealed itself as a new and formidable enemy of mankind. By destroying the staple food of a human society already sick from economic causes, it brought about more of death and suffering than any other disaster since the Napoleonic wars. . . . The passage on June 26, 1846 of the Act which ultimately repealed the Corn Laws was perhaps the most significant single event in the history of the British Empire. The Duke of Wellington's comment was: 'Rotten potatoes have done it all; they put Peel in his damned fright'."

Now the point that matters is this: these were the most severe epidemics of potato blight that have been recorded, and they occurred in the eighteen-forties, the period that historians refer to as the hungry forties. Admittedly Liebig in 1840 began to talk about chemical plant nutrients and before 1845 Lawes had begun to make superphosphate, but no one can seriously declare that fertilizers were very widely used at that time. Their use was experimental and quite infinitesimal against the background of universal cropping on dung and lime. It is certainly not true to say of this disease that its ravages have been greater since the general introduction of fertilizers.

It took some 40 years of research work before a means was found to combat potato blight. In those 40 years the disease took its toll according to whether seasonal conditions minimized it or encouraged it. The discovery of Bordeaux mixture in the late eighties led to its gradual acceptance as a preventative method. And this introduces another point. The humus school frequently says that the *poison spray invariably follows chemical fertilizers* and it certainly might seem from a cursory glance at the facts about Bordeaux mixture that spraying, having become a somewhat general practice among progressive farmers by 1900, might

be due to the use of fertilizers. The *fact* is that spraying would have been introduced in 1845 and 1846 and in all the years after if only this method had been known and established then; incidentally the history of the British Empire might have been much changed. Spraying came along at the turn of the centuries because it was not available before, and in this instance it is sheer nonsense to say its necessity has the slightest connection with fertilizers. It was a new method to combat a plant disease whose severest attacks happened before the fertilizer era, whose further attacks occurred without any relation to an increasing use of chemical nutrients.

When I have used this argument elsewhere I have been told by humus school opponents that it is an unfair comparison, that comparisons should be made only under reasonably similar conditions. However, the argument is not comparative, it is factual—all that I seek to show is that disease occurred with epidemic seriousness when fertilizers were *not* used, when humus manuring was the rule. It is in any case a much more reasonable argument than the humus school's frequent comparisons of the agricultures of vastly different world areas, peoples, and climates.

The issue that indeed might be raised is to ask why the manurial policy then followed did not give better immunity to the attacks. The answer is that 1845 brought a wet summer, and the favorable condition for blight spores is wetness. Eighteen-forty-six was not so wet but the fungus was well established from the preceding victory. The conditions were too powerfully in favor of blight for any nutritional system to overcome. Today we may thank God that we know one sure answer to this problem—the copper-type spray, a chemical method. A more modern method may come from the plant breeder so that eventually we shall have blight-immune varieties; but again this has nothing very directly to do with humus or with fertilizers.

While talking about potatoes we might as well cover some of their other troubles at the same time. There is the eelworm nuisance, admittedly often serious; this too has been blamed upon fertilizers by the humus school. Balfour presents a very

plausible argument for considering that eelworms will be discouraged and reduced in number by humus sufficiency; she points out that many soil fungi are able to catch and consume living mobile organisms by a kind of fly-paper technique, the fungus mycelium giving out a sticky substance which holds the eelworm, this being followed by the penetration of the eelworm's living body by the fungus. This horrible natural struggle is well authenticated by workers who have specialized in fungi, and Lady Eve Balfour has drawn valuable attention to the fact that over fifty varieties of these predaceous fungi have been recorded. From this it is argued that an all-humus policy will defeat the eelworm because abundant humus will encourage the fungi and the pests will be routed in a survival-of-the-fittest struggle. This seems reasonable enough as a pro-humus theory, but it is not also evidence that the chemical nutrient supply will discourage the fungi concerned. Again we face the same cross-purposes issue.

It should be added that Lady Eve Balfour also reports evidence that badly infested eelworm ground has been cleansed rapidly by compost dressings, so that this pro-humus theory is more than a speculation. This case came from Rhodesia where infested tobacco land was treated with a five-ton per acre dressing of compost; in the first year the trouble was reduced, and in the second year, without further application, the eelworms had gone.

The humus school has been particularly active with the eelworm charge against fertilizers. Sir Albert Howard has more than once quoted the decline of certain English potato-growing areas from high yields to abandonment owing to the increasing eelworm attacks. When these areas were orginally plowed their natural fertility supported high cropping—gradually fertilizers were needed—and gradually too the eelworm trouble increased; in brief, that is the argument, and at first sight it seems formidable. However, there are other opinions. Dr. Sanders in his *British Crop Husbandry* gives a very concise statement of more orthodox ideas about this land pest.

Eelworms are always present. Whether they seriously affect crops depends upon the number per acre. Complete failure may

arise when that number gets up to 200,000 millions. The modern tendency for such large eelworm populations to arise is blamed by orthodox opinion not upon the increased use of NPK fertilizers but upon the decreased practice of rotation. Different strains of eelworms to some extent attack different crops, the potato worm is specific to the potato, the sugar-beet worm to sugar-beet, etc. By rotating crops eelworms in the past have been well controlled; for however rapidly they may multiply in one season, they will be starved out in the next because their necessary food is not kindly being placed there by man. In recent times, however, rotation has ceased to be economic because it is a long-term method of production and farmers often have not had the capital or the bank managers to let them risk long-planning for the fluctuating markets of a badly organized world. Sugar-beet, subsidized so that firm prices can be obtained, is clearly a safer annual crop economically than a four-course rotation three of whose crops might only lose money. I stress this economic background to rotation because it is not fair to suggest that farmers dropped rotation through stupidity. To return to the eelworm, it is clear that nothing could encourage it more than non-rotation, more than the year-after-year cultivation of crops the eelworm thrives upon. And that is just what happened in modern times in those areas where successful potato-growing has declined sometimes to the point of utter failure. It has even been suggested in connection with this that but for fertilizers the decline would have been more rapid, fertilizers often getting the plants forward enough and strong enough to resist attack better. The orthodox cure for eelworm trouble is crop rotation and the gradual starvation of the pest, this often taking several years when the attack is bad.

This view is supported even in law. Sanders reports that, "heavy damages have been awarded against a farmer for ruining his landlord's property by growing potatoes too frequently." German sugar-beet factories had to close down because crops failed through eelworm attack. Here the sugar-beet factories, fearing the possible blow at their fairly young industry, put

clauses into their contracts with farmers to prevent sugar-beet cropping in two successive years on the same field.

So the eelworm's modern expansion seems more likely to be due to non-rotation which is in turn due to economic world causes than to fertilizers. Is humus-manuring the cure? It might be worth trying on a long-term crop-after-crop scale, but its success would depend upon whether the killing activity of the encouraged predaceous fungi could continue to exceed the rate of eelworm expansion encouraged by the regular supply of a suitable crop for eelworm feeding. I cannot resist the comment that it would be odd if this school of thought, so frequent in its denunciation of mono-culture and non-rotation, discovered a means by which that practice could be made safe! In any case, there is probably no crop more regularly supplied with both manure and fertilizers jointly than the potato crop, so that humus deficiency should not be often associated with its cultivation. Even unwarrantably assuming that fertilizers help the eelworm by not helping the antifungi (for there is no evidence for this specific charge), it would be interesting to work out (a) the increased tonnage of potatoes produced by the modern use of NPK and (b) the losses through eelworm. After all, we do seem to be growing more potatoes. However, as we have seen, even if we abandoned chemical nutrients, we should still not be obviating potato famines unless we blotted the non-chemical sheet with copper sulfate sprays.

Another potato trouble that has turned up in this war and which turned up in the last is the blackening of tubers on cooking. This *is* due to nutrient trouble. Several independent research teams have shown that it follows the supply of too much nitrogen and too little potash. It is therefore attributable to the misuse of fertilizers. Its wartime appearance is interesting. In wartime grassland is plowed up and potatoes are in great demand. The two facts go together rather frequently. If the turned-in turf decomposes quickly, or in the second year if this process is normal, the ground is naturally rich in nitrogen. A third wartime fact then intervenes. Potash is almost entirely imported,

and potash quotas are in wartime rather on the low side. Therefore, a fertilizer is apt to be used which is relatively high in nitrogen, low in potash—for this potash-loving crop. Add the natural flush of nitrogen, and you have a dangerously high nitrogen to potash ratio—and the potatoes blacken on boiling to the detriment of the housewife's morale. Those who have suffered from this domestic disaster in recent times may have unfairly blamed the farmer; the true explanation is that sometimes—and this is no unmentionable secret now—potash in precious cargoes, and perhaps the men with it, have gone to the bottom of the sea.

However, the potato seems to have carried us very much too quickly from last century's *hungry forties* to the *bloody forties* of our own more enlightened days. We must step back again into those times when income-tax was measured in pence and fertilizers were but an idea.

It was not only potato blight that made the eighteen-forties hungry. The vines of Southern Europe were attacked by mildew between 1848 and 1851 on a scale that caused panic among the growers. The English hop-gardens suffered a much severer mildew invasion than they had ever known. Thirst too seemed threatened. Here, however, a remedy was speedily found—the mildew could be controlled by sulfur or lime-sulfur dustings or sprayings. Again the point must be made—here were intense epidemics of disease, so intense that for a time it seemed that the specific crops would be totally destroyed over wide areas in Europe—but occurring before the days of fertilizers. These were days when farming practices and civilization's habits insured a greater supply of manure per acre for most growers. And let it be noted that with these mildews the poison spray to check them and stop them came well before the fertilizer era, simply because it was discovered quickly. Chronologically there is no spray-following-fertilizer history here. If fertilizers were used on vines and hops in 1855, by which time sulfur-spraying was a general practice, the actual amount of use was insignificant and isolated.

With the cereal crops Large records that there was plenty of trouble in the pre-fertilizer era. The wheat crops of France in

1760 failed through the *bunt disease,* sometimes called *stinking smut.* The remedy for this trouble came remarkably early in the history of agricultural science; in the early seventeen-fifties Tillet, a French amateur scientist, proved by experiment that the *bunt* was a seed-borne disease, and within a few years it was shown that seeds could be cleansed of the dormant fungus by treatment with lime. Later copper salts were added to the lime with even better results. Here again severe plant disease was recorded before the time of fertilizers on bigger scales than we have to face today; and again the chemical remedy was discovered well before fertilizers.

In 1868 the French vineyards faced phylloxera, a disease that struck on epidemic scale almost as devastatingly as the potato blight of the forties. The vines died; root examinations showed deformed, nodulated conditions. It was a contagious insect attack upon the vine roots. The interesting thing was that American vines seemed immune to these attacks, whereas the French vines, for all their wine-making superiority, were losing the battle with these root lice. So the growers, unable to produce good wine from the American varieties, ingeniously bred new varieties from American roots with French-vine qualities grafted on. Gradually, but certainly, the epidemic was defeated. To quote Large:

"This was to be the salvation of viticulture and the wine industry, not only in Europe but also in the New World. . . ."

"But the great defensive measure, which ultimately saved the vineyards of France . . . was that of starving the invader. And the way of using this weapon of starvation was to build up, by selection, grafting and breeding new varieties of agricultural plants. . . ."

Here, at any rate, was no poison spraying remedy; and, at the same time, no evidence that an industry's greatest recorded attack from disease had in any measure followed the use of fertilizers.

The British are a tea-drinking nation. The French and the Americans prefer coffee. It is as well to remember that in Dr. Johnson's and Hazlitt's time, it was in the *coffee*-houses that

talkative society gathered together. Coffee might well have become the British national drug-supplying beverage had it not been for an epidemic of disease that swept away the plantations of Ceylon in the eighteen-seventies. Coffee rust reduced the average crop from 4.5 hundredweights per acre in 1871 to 2.0 hundredweights in 1878, and the loss was about two million pounds per year. A commission was set up, and this committee asked for expert research workers. Large records that this step was taken too late—by the time the research workers had been appointed and had reached Ceylon, the industry had all but collapsed. However, Marshall-Ward carried out some important investigations which were of historic importance to plant pathology even if they were not able to save the planters from ruin. He drew attention to the fact that the disease had spread among the cultivated plantations because the natural system of mixed crops had been abandoned, because the airborne spores could find a host in every adjacent plant whereas Nature's heterogeneous farming methods would have seen to it that no large area was given over to the single cropping of one kind and one variety of vegetation. To quote Large, "specialized cultivations *invited* epidemics of plant disease." The other advance made by Marshall-Ward was the idea of chemical *pretreatment* of foliage to deal with a fungus that was difficult to destroy once established, that is to say, spraying leaves with a chemical which would prevent a likely spore attack rather than spraying with a chemical which aimed at destroying after invasion. If Marshall-Ward failed to save Ceylon's coffee, he succeeded in opening new ground in the theory of disease causes and disease treatment, for later the pretreatment principle was to produce the solution to the potato blight problem.

The troubles of Ceylon spread farther. The Arabian coffee plantations succumbed too, and Brazil became the world's coffee grower. Ceylon abandoned coffee and planted tea, and England by a strange imperial coincidence took more and more to the tea habit! Now these things happened sufficiently after the early days of fertilizers for it to be a *possible* argument that the use of

fertilizers had something to do with the incidence of this rust disease. *Possible* is the most that can be said, chronologically possible. Marshall-Ward, in his analysis of the causes of the Ceylon disaster, laid the blame upon too much specialized cropping, upon the creation of conditions that provided maximum opportunity for the spread of fungus spores, and upon the exposure of the plantations on the hillsides to seasonal winds which mass-transported the spores to this paradise of spore germination. He did not include in his diagnosis any connection of the disease epidemics with nutrition, either with NPK nutrition or with humus nutrition.

When we begin to examine the records of plant disease in the twentieth century, we are well within the era of chemical fertilizers. We can no longer prove that severe outbreaks cannot be due to fertilizers on a kind of crime-story alibi argument. However, the plant disease research workers do not seem to spend a great deal of time on this idea that fertilizers and disease are connected. The lines on which they are working—and progressing—are of two kinds. The destruction of the direct cause, the fungus or the insect, by chemical warfare aimed at the weakest point, the bottle-neck, in the life-cycle of the organism—and the remedial or preventative method of the spray or the seed-dressing, remedial if carried out after infection begins, preventative if carried out before infection is likely. Second, the development of varieties which are immune or relatively immune, varieties cross-bred from those which seem to weather the attacks and those which have more desirable cropping qualities but which are unhappily very prone to attack.

The *recent increase* in the virus kinds of disease is not unexpectedly attributed to fertilizers by the humus school. Because recently there has been much more attention paid to virus diseases, we are apt to assume that they are more prevalent, when in fact they may well have been equally as prevalent for centuries. Repeating an earlier point of this argument, it is not until diseases can be sharply diagnosed that we realize how often they occur; before virus diseases were specifically studied, their symp-

toms may well have simply been written off as troubles due to the weather or to bad seed or merely to an Evil Providence. Large indeed records that a virus disease affecting peach trees was known in America in 1807. However, virus disease research did not begin until the late nineteenth century.

A virus is still a somewhat unknown quantity. Viruses are either substances or organisms, and they are smaller than microbes; some have been isolated as crystalline bodies, but even that development has not made those who specialize in this field of inquiry feel able to declare that viruses are non-living entities. Hugh Nicol in *Microbes by the Million* suggests that the virus may indeed be the border line between animation and inanimation. However, whether living or dead or chemical, the virus is contagious and when one virus-infected plant contacts another, perhaps through successive handling by gardening fingers, the disease spreads.

A quotation from Large emphasizes the view that it is virus research rather than virus diseases that has increased in recent years.

"Certainly the plant virus diseases appeared to have spread alarmingly, and to have become more prevalent since the beginning of the century (twentieth). One might speak of *the advance of the viruses* as of *the advance of the fungi* and fairly attribute it to the same world causes; but the virus diseases of plants were new in nothing but name. The *leaf roll* of the potatoes, when it was at last disentangled from the other diseases to which it had a most misleading partial resemblance, could be taken as one of the very best examples of a modern virus disease, but as the chief part of the trouble once known as the *curl* it had ravaged the potato crops in England during the eighteenth century —long before the blight was ever heard of; the peach yellows had a long history in the United States; the *tobacco mosaic* had been recognized as a destructive disease for over 50 years; and the breaking and streaking of tulips, found during the twenties to be a consequence of virus disease, had been depicted in the paintings of old masters of the Dutch school."

Virus diseases are not only associated with immediate disease—they lead to degeneration, to falling yields where seed is saved season after season. It was found that viruses were spread by being carried by insects, by aphides; it was then found that in areas where the conditions did not suit aphides—cold, wind-exposed areas—virus diseases were negligible. The Scotch seed potato trade owes its reputation to this fact; for seed potatoes, raised in areas so unfavorable to aphides, were not carriers of virus—they were *clean.*

On the whole, virus diseases have not been troublesome on the same scale as fungus-caused diseases. The virus disease seems to be a kind of sub-health, a crop-reducing disease and not often a crop-destroying disease on a total scale. Many gardeners will have noticed mosaic discolorations of marrow plant leaves or cucumber plant leaves, yet will have secured a fair crop, if not a bumper crop.

How does the theory that these diseases are caused by fertilizer practice stand up to the independent evidence of virus research? Since virus diseases are spread by aphides, then it may be that the idea simplifies itself to the fact that aphis attack is increased where fertilizers have been used. There is no evidence to establish this, but Lady Eve Balfour has put forward a theory to explain why insect pests of this kind may attack fertilizer-fed crops more than they attack crops raised upon humus manure only. It is a theory resting upon a number of unestablished propositions.

1. The mycorrhizal association having been inhibited by chemicals, the plants have not been properly fed and therefore do not contain the full quota of nutritive values as designed by Nature.

2. The insects go on attacking the foliage of such plants because the food provided is *lacking*—they go on eating in the hope of finding what is actually not there. Whereas, when they attack humus-raised plants, they are soon satisfied, and, being satisfied, their attack is lessened.

This proposition is somewhat startling. It is a complete reversal in logical development from most other arguments of this school.

It suggests that *bad conditions for the development of living organisms will lead to their greater activity!* It is remarkably different from the argument that fungi cannot develop when there is insufficient humus present. It is amazingly opposed to the natural law of the survival of the fittest in which so often the *fittest* means those organisms which enjoy favorable conditions.

To be fair, it should be said that Lady Eve Balfour has put forward this theory somewhat tentatively as an explanation of the fact she believes, that insects attack chemically raised plants more than humus-raised plants; when it might seem more reasonable to expect that insects would prefer the supposedly healthier plants. It is not universally accepted that humus-raised plants are less attacked than those raised upon fertilizers. It is possible that softer growth caused by excessive nitrogen application may be less resistant to various attacks, but beyond this special case of fertilizer misuse the argument cannot be accepted. The absence of any explanation other than this most unplausible speculation may well be due to the fact that explanation is being sought for something that is not actually true, and therefore not requiring interpretation.

Unless, then, there is practical evidence to prove that fertilizers do induce bigger and more damaging attacks by aphides, or unless there is any logical argument to suppose that this can be so, it is difficult to see how fertilizers can cause the greater spread of the virus diseases. Indeed, on a more general argument that embraces chemicals both as fertilizers and as insecticides, there is a lot to be said for the chemical as a virus reducer; for, if young plants are sprayed with a chemical which will deter or destroy aphides, then there will be much less chance for virus infection. And this is indeed a frequent practice in market gardens.

This, however, does not rule out the argument that crops raised on humus may be more resistant to virus disease itself, perhaps because of the mycorrhizal association. At least, there have been claims by compost enthusiasts that humus manure

applications have minimized and even completely cured virus diseases. However, this again is one of these pro-humus arguments that is not *ipso facto* an antichemical argument. It may well contain plenty of truth. It may well be that humus deficiency leads to plant growth that is more easily deranged by virus infection, but need it be repeated once again that fertilizers are complementary to humus and to use them in humus-deficient soils is to misuse them?

The main point of the humus school does not hold. Virus diseases have not increased since fertilizers came into more general use—the factor that has increased is our knowledge of virus diseases which is expressed by a much greater attention to them. Virus diseases were well known and widely experienced long before fertilizers were used at all. Even now very little is known about the viruses. It is a comparatively young field of research. Neither the humus school nor the orthodox school can afford to argue outside the very small area so far cleared by research. If there is perhaps some evidence for the view that humus sufficiency may aid resistance to virus infection effects, there is none for the view that fertilizers increase infection or cause infection.

In the course of discussing virus troubles we have had to anticipate to some extent the question of insect attack, and the argument put forward by Lady Eve Balfour has already been stated. Most attacks by non-micro-organisms on plants and trees are describable as damage rather than disease. The humus school maintains that this kind of trouble is increased through the use of chemical nutrients. Again it may be true that humus sufficiency will provide plants more able to suffer a certain amount of damage, more able perhaps to repair such damage speedily—such an idea is reasonably plausible. However, the fact that it does not need fertilizers at all to induce severe pest attacks and that these epidemics can thrive under fairly *natural* conditions can be established by almost any visit to an old fruit orchard attached to a farm. To quote Large again: "Such neglected orchards were veritable jungles of parasitic fungi and insect pests.

"The trees also suffered much from partial starvation, for

there was a curious notion among the English farmers that fruit trees were not as other plants. They were supposed to have very deep roots which could draw all the nourishment they required, indefinitely, from the interior of the earth. They were rarely given any fertilizers or manure other than that which they got back from lean pigs turned loose to fatten off the grass beneath them."

The humus status of these orchards is perhaps difficult to decide. The organic matter status must be high—for grasses, leaves, and some manure would always be falling back as residues into the soil. The amount of humus in the soil provided by this supply would depend, however, upon the aeration factor. The earthworms should serve one of their main purposes here. If such orchards cannot be fairly describable as good examples of humus sufficiency, then at any rate within the terms of the humus school's thesis they should be better off for humus than orchards where nutrients are mainly supplied by chemical fertilizers. Yet few would deny that these orchards are almost all fruit-tree sanatoria or graveyards, largely owing to the cumulative and unopposed attacks of fungi and insects.

The modern commercial orchard generally presents a very different picture. Here fertilizers of organic and inorganic kind are regularly used; and specific chemical sprays drench the wood in winter and the foliage in spring and sometimes the young fruit after a blossom-set. It cannot be denied that in the Cinderella orchards of the diversified farms other aspects of fruit cultivation are neglected too—pruning, non-removal of diseased fruit, etc., but, even making considerable allowances for this, the modern orchard with its use of chemicals both in the soil and upon the trees is convincingly freer from pest attacks. The combined use of fertilizers and sprays in fruit cultivation has indeed led to the charge that sprays and NPK chemicals go together, but I suspect that it would take a great deal more evidence than we have today to persuade a modern commercial grower with a lot of money sunk in his plantations that he would do better to give up fertilizers and sprays. Had the old orchards which never received

fertilizers kept cleaner, this antichemical argument might be more convincing.

Further, as a result of the war, and as a result of the wartime food-production policy which quite early gave fruit a low priority for rationed fertilizers, many of these modernly managed orchards have deteriorated in condition, especially through potash shortages. While this has not affected the income from fruit sales owing to the wartime demand for any quality of produce, it has certainly been a bad thing for the capital value of the orchards. Such cases as these, which are I believe quite common, are evidence for the view that fungus and insect attacks upon fruit make more headway when fertilizer applications are reduced; though possibly the shortage of labor for adequate chemical spraying is also a contributory cause.

As a tail-piece to this discussion about fruit, surely one must be very open-minded indeed to believe that apples will be less burrowed into by grubs and suchlike if the tree has been manured only with compost. I may be dogmatic in raising this question, but I am unable to swallow the idea that the moth-grub or maggot is influenced by the soil-feeding of the tree; once the fruit is there for the taking, the maggots will do their invading unless other methods unconnected with tree-nutrition are adopted to control them. After all, many keen gardeners used farmyard manure in plenty for their trees years ago, and cultivated the soil so that the manure had good aeration for humus conversion; yet, without additional methods of treatment such as spraying and grease-banding, they could not solve the problem of losing a good deal of their crop to these pests.

We cannot run through the whole encyclopedia of diseases. Enough has been said to demonstrate that, while there may be in many cases some justification for considering that humus sufficiency is a factor for health and better resistance to various kinds of attack, there is little evidence to support this other opinion that chemical fertilizers induce disease or make disease worse. The case for fertilizer defense could well rest at this point. For the sake of a few more pages, it might be profitable to con-

sider what actually may be the causes of disease and the conditions that encourage disease. To say that chemicals cannot be blamed is a negative and no more; defense would be more positive and real if the true culprit or culprits could be indicated. A man charged with murder might clear himself by argument on alibi and motive lines, but he would be much more surely cleared if it could be proved that some other definite person really sprinkled the arsenic in the milk pudding.

The final chapter in Large's book might well follow here in its entirety, and would do but for the fact that one writer must exercise a little moderation in the extent to which he quotes another. Large points out that from time to time various universal panaceas for plant diseases have been urged with great enthusiasm—such policies as enclosure, turnip growing, rotation, drainage, the NPK chemicals, humus via compost, the trace elements, and so on. However, it is necessary to probe deeper into the fundamentals of agriculture, deeper than accessory actions or passing emphases of farming. Here is a most significant paragraph from Large, so significant that it would be an impertinence to summarize it. (It should, by the way, be explained that Large adopts the historical past tense in his book, even when writing of more or less current matters.)

"All agriculture was artificial. There was nothing more artificial in the world than a field of cultivated potatoes. For what was agriculture, after all, but an attempt to strip areas of the earth's surface of its wild mixed flora and fauna, and to reserve such areas exclusively for the growth of plant prodigies, most of them brought from foreign lands, and all of them chosen for abnormalities of special utility to man? This agriculture, this exclusive tending of vegetable freaks and monsters, was necessary if the human species was to survive. But as it was necessary, so there was nowhere at which it was philosophically possible to draw a line, and reasonably say that up to such and such a level in its historical development agriculture was *natural* and right, while all beyond became *unnatural* and wrong. No one could say, for example, that it was natural and right and proper to

put lime on sour land, as their great-great-grandfathers had done, but wrong to stimulate the growth of plants with synthetic sulfate of ammonia. Both lime and sulfate of ammonia were products of the chemical industry; both were ultimately derived from the waters and rocks of the earth and the constituents of the atmosphere. The best that man could do at any time to defend the health of the hypertrophic agricultural plants that in his cunning he had sought out or made, was to apply to the work of rearing them the *whole* of his science. . . ."

This passage hits the real nail squarely on the head. Agriculture is fundamentally unnatural, it starts off with unnatural soil treatment and most plants today are kinds specially bred for man's own purposes, for the most part long removed from their natural birthplaces and rapidly stepped up in evolutionary changes not by Nature but by man. With such a background, how can anyone lay down rules that accessory actions in cultivation and plant nutrition should be only those kinds of actions which are themselves natural? Indeed, in talking about *being natural* in agriculture, are we not often merely at verbal cross-purposes? To many people nothing could be more *natural* than a field of potatoes neatly set out in rows. Certainly on a train that leaves the built-up huddles of a city and then runs past potato-fields, the arable crops of farms are comparatively natural; the enormous difference in naturalness between the potato fields and some adjacent permanent grassland or scrub is not so apparent without thought. Natural is a bad word—it has discarded most of its meaning. There is nothing paradoxical today, for example, in saying that it is natural for a girl of eighteen to use powder and paint. However, we had better get back to the potato. . . .

The potato was originally a wild Mexican plant. Imported into other climates and soils, new varieties were bred in a perpetual effort to obtain potatoes which could in these new conditions provide bigger crops or earlier crops. It is no good criticizing this procedure—it is, on the whole, the kind of thing man must do to remain the predominant form of life on earth. Despite coffee-burning and fruit-dumping and similar economic methods

of distribution, there has never yet been a time when all people in the world had enough food. Man must develop food-producing practices by his own ingenuity and he must risk making mistakes in the process—the remarkable thing really is that he has not made more mistakes. It is natural for man to interfere with Nature—he must. Nature would leave the wild potato in Mexico. There it would grow among Nature's mixture of symbiotic vegetation, always in conditions under which it could survive against its enemies. Alter the geography, the variety, the method of cultivation, and the ability of the plant to defeat these enemies *without artificial help* is lost. Marshall-Ward's diagnosis of the Ceylon coffee disaster applies equally to every field of potatoes—unless something artificial is done to beat the blight fungus, the spores can jump from one plant to another as they could never do if the plants grew in wild clumps separated by gaps that would mean starvation and death to most traveling spores. The necessary practice of growing whole fields of one crop, and probably if the locality suits that crop specially well, of growing numerous adjacent fields all with the same crop, this artificial man-made procedure is a creation of maximum opportunity for mass-infection. And it is also agriculture.

To ask what is the cause of the potato blight spores is to raise a query beyond the powers of human understanding. You might just as well ask for the cause of the original Mexican wild potato. The cause of disease cannot be pressed too far back unless the whole argument shifts into the metaphysical. In a practical sense the cause of the blight, recognizing it as disease only when it starts to be significant in amount, is the development of conditions under which the spores can multiply at an uncontrolled rate. By artificially changing the conditions of potato-growth from those conditions under which it could overcome the spores, we introduce the further problem of dealing with spores that can then sometimes thrive (especially in wet weather) better than the crop. To preserve the good effects of his artificial venture man must try to compensate for the bad effects with further artificiality. It may be that the further artificiality then intro-

duces further troubles even though it compensates for the former ones; thus a fruit spray to control some fungus or insect attack might kill off too many pollinating insects; then man must think again and devise some more selective method of solving his original problem.

It seems, then, that the causes of disease are deeper rooted than the antichemical thesis of the humus school would suggest, that they start with agriculture itself. Since diseases may increase through every act by which agriculture moves toward man's purpose and away from Nature's, it is difficult to see why fertilizers should be any more causative than other practices; indeed, we have seen that single-cropping in one field, a practically essential economic step, is likely to be a much more fundamental cause. With this argument added to the other more factual arguments—in which we have found so little evidence to blame chemicals for increases in disease—this defense of fertilizers can be left. Sir Albert Howard's point that Nature is the supreme farmer is not enough; Nature farms for her own purposes, she does not farm for Smithfield or Covent Garden or even the Milk Marketing Board. We must make many changes and we must keep on propping up our changes with further devices; each of these steps must be judged on factual evidence, on the balance of its merits or demerits. It is useful to study Nature as a model, but it is not necessarily a slavish law to be followed unless the practical evidence also points overwhelmingly in the same direction. There must be many compromises between Nature's methods and ours; for her purposes and ours are significantly different.

Summing up, there would seem three possible conclusions. I am not stating them in order to force them into anybody's mind; rather, I would suggest that other books should be read and other presentations of evidence and deductions considered too. For guidance only the points below are set out.

1. Humus is an important part of plant diet and a sufficiency is necessary if plants are to be as virile as their other conditions permit.

2. There is no evidence that the incidence of diseases or pest

attacks is increased through the use of chemical nutrients; on the contrary there is evidence to show that resistance to some diseases is increased. Such chemicals should, however, be used in a balanced way and with complementary regard to point 1.

3. Although malnutrition may cause lower resistance to infection and attack, the primary reasons for epidemics are bound up with several other factors unconnected with nutrition. It follows that good nutrition is only one method of control and that it may often in itself be insufficient.

The following digest of a paper by R. R. Follett-Smith from the Proceedings of a Meeting of Sugar Technologists in the British West Indies (1943) seems highly relevant not only to the subject discussed in this chapter but also to the subject of the next chapter, the food-values of crops.

"From 1879 to 1903 the yield of sugar in British Guiana averaged 1.6 tons per acre. Since 1938 it has averaged 3 tons per acre. During the period 1926–1939, when fertilizer supply was unrestricted, there was a rising trend of sugar yield and a close correlation of Colony sugar yield and rate of application of sulfate of ammonia, *yet practically nothing has been done during the past 40 years to augment the organic-matter content of the soil. The Colony's cane has been very free from disease for 20 years.*"

CHAPTER XV

FOOD VALUES AND THE CHEMICALS

"The nutritional requirements of any man, woman, or child are best satisfied by a continuous series of square meals. Knowledge about the minimum of essential substances needed for life and growth finds a better application in the calculated efficiency of a highly productive agriculture, of which the chief aim should be the production of the greatest amount of food value for the expenditure of labour made." DR. J. S. D. BACON, 1944, *The Chemistry of Life* (Watts).

NUTRITIONAL QUALITY is just as much a measure of farming practice as crop yields. The loaf, after all, is one objective of the wheat crop. However, this property of foodstuffs is not nearly so easy to assess, though we are beginning to know something about it. Two wars, in both of which food supplies have become major aspects of war conduct, have taught us a great deal; especially is this true of the 1939–45 war in which we have profited very greatly from mistakes of 1914–18. Politicians, faced with the problems of keeping populations adequately fed, have had to listen to scientific opinion. Finding that public tastes and habits often bear little relation to sound nutritional principles the politicians have had to stimulate public interest in these matters. Lord Woolton's contribution to practical nutrition has a wider significance than his more obvious and immediate *fair-shares-all-round* achievement in the difficult days. Mr. and Mrs. John Citizen have not only learnt a lot about blast and fire-fighting since 1940; they have learnt a great deal about vitamins and fresh foods and so on.

Malnutrition is not merely a matter of insufficient amounts of food. It can be—starvation is obviously malnutrition. Malnutrition can turn up where poor foods are most bulkily eaten. Food,

like soil, is a widely general term. The human body needs a complex mixture of many complex substances. Two dynamic equilibria must dovetail together—the soil and the plant for one, and the plant-produced food and the human body for the other. To bring about this dovetailing is not easy. Nutritional and agricultural sciences are both imperfect sciences, the things they investigate are not stationary, the conditions of their experiments are difficult to control owing to the large number of external influences involved. Agriculture is dependent upon traditions and upon world economics; people's diets are dependent very frequently upon customs and fashions and wage packets. It is therefore a very precarious venture to try to generalize about people's health and its connection with the manner in which their food is grown. All the imponderable factors of both equations are present. We have to consider not only the extent to which health depends upon the quality of food consumed, we must assess also how much health depends upon other factors too. To make any connection, we must not only consider how much food quality depends upon methods of soil treatment, but we must assess other factors that are concerned in this. Sweeping statements on this subject should be analyzed to see what they have left out.

The humus people argue that food raised with fertilizers is less nutritious. Orthodox science replies that this is quite untrue, that foods raised (a) on organic manures and (b) on NPK chemicals have been carefully compared and no nutritional differences found. To this the humus school often replies that it is not possible to analyze for true nutritional value since there are probably many still unknown factors concerned—therefore, *chemical* foods may be deficient in these unknown factors as compared with *humus* foods. Well, of course, this is a very intangible argument, and the only thing to be said is that the thesis too is intangible.

To begin with we had better stick to the light of contemporary knowledge. Some years ago the nutritional team was protein, carbohydrates, roughage, fats, certain minerals, and vitamins A,

B, C, and D. Now B is a total association of B_1, B_2, and so on—E has been added and P is being studied. It is quite true that we probably do not know how many kinds of vitamins there are, nor whether some of those we have named are just one substance or, like B, aggregates of several. Nor are vitamins the only trace substances needed for the proper working of the body. There are the enzymes, organic complexes which perform catalytic functions; that is, they make other substances react, e.g., most of the biochemical changes known as food digestion require the presence of enzymes. Some of these are manufactured by the body itself—digestive enzymes are provided, for example, in saliva; others may have to come through food supply. In any case, those made in the body possibly depend upon the supply of certain specific substances in the human diet.

While recognizing that nutritional research is young and still perhaps only standing on the fringe of things, it must be realized that for most of these known factors of food value biochemists have definite tests by which they can be measured. There are two kinds of test—biological and chemical. The biological method is the real and decisive method; by it the effects of the food under test are measured by the responses of a laboratory animal to the food. Rats are frequently used in such analyses. This kind of testing may seem far from precise, but by carefully controlling all other factors than the one food value under examination, biochemists have been able to develop this kind of work to a high standard of reliability; that is to say, when the same tests are repeated, good consistency of results is obtained. Coupled with this rather cumbersome and expensive method is the purely chemical test, by which the amount of some particular food value is measured by some chemical reaction it will undergo through the addition of other chemicals. This easier kind of test is, however, based upon the biological test because its results are not regarded as reliable unless, in several kinds of foods, these agree with those of side-by-side biological examinations. When this general level of agreement is reached, it is considered that the chemical test gives a true reading of the food value.

There is often this possible source of discord—the biological test will measure all nutritional effects of the vitamin concerned; the chemical test is based only upon a change occurring to one property of the vitamin. For many foods the two values may agree accurately; for some foods deceptive results could turn up. To make this clear, suppose that a vitamin X is actually X_1 plus X_2 and X_3—though this is not clearly understood or even suspected. The chemical test may measure only some property of X_1. With a number of foods agreement is given in the two tests because these foods all contain the whole X association; but if a food comes along which contains only X_1 and not X_2 or X_3 the biological test will give a lower figure (the real measure) than the chemical test which is based only upon X_1. Therefore in all important measurements of vitamin values, we should rely upon the biological check as well as upon the simpler chemical check.

All of which is a digressive preamble to show as briefly as possible that we must not always expect clear-cut black-and-white evidence in any major issue about nutritional values. There is plenty of room for speculations and doubts, and the heretic can reckon to have quite a good chance in an argument with an orthodox believer. Indeed, the heretic with persuasive debating powers could reckon to win the day over the orthodox man who dares only to express his views with caution.

In the light of the known and measurable food values, crops raised with fertilizers and similar crops raised only with organic manures have been deliberately compared, and no inferiorities have been found by official research stations. To quote Sir John Russell:

"We have searched diligently for evidence that organic manure gives crops of better quality than inorganic fertilizers, and so far our experiments, made jointly with the Dunn Nutritional Laboratories at Cambridge, have all given negative results. No difference has been found." (1939.)

"From time to time there has been much discussion on the question whether organic or inorganic sources of plant nutrients are best. It seems strange that in this twentieth century there

should still be people who think that ammonia derived from organic matter differs in some subtle way from ammonia derived from gas liquor or produced synthetically. I know of no evidence that organic manures produce healthier or more nutritive crops than inorganic fertilizers." (1943.)

The former quotation is of greater specific importance for the Dunn Laboratories referred to are laboratories where the *biological* tests for vitamins are carried out; in England the issue could scarcely have been taken to a higher court.

There is, then, this positive fact. Any differences, if there are differences, must lie in the realm of the unknown. Since in all the known factors of food value fertilizer-raised crops are not inferior, why should we suppose that they will be inferior in any other factors? For a specific plant, or part of a plant, probably has a certain normal balance of these nutritive factors; if the intervention of chemical nutrients has clearly not distorted quite a number of these factors, it is likely that no distortions have occurred with other factors that happen to be unknown.

Nor should another much more positive argument be forgotten. If the known nutritive values of crops raised with and without chemicals are the same, the total nutrition provided through the help of chemicals is greater; for in most cases the crop yield is greater, as we have seen. These nutritive comparisons have been made on a sampling basis; but the real comparison is not that of vitamin value per gram of food, but total vitamin value per crop per acre. This aspect of the connection between fertilizers and nutrition must be forcefully stressed, and this last argument can be expanded. In chapter three a Jealott's Hill experiment showed that the protein value of a grass crop had been increased by fertilizer application to a greater extent than the actual increase in yield.

However, the humus school's case does not rest entirely upon the intangibility of the unknown. Both Sir Albert Howard and Lady Eve Balfour paint a broad canvas of modern malnutrition, of general sub-health; and it is implied and sometimes boldly declared that this is due to the general reliance upon fertilizers in

modern cropping. The trouble with this argument is that it is a very mixed one. One can agree about the general malnutrition and the sub-health, and yet totally disagree with the deduction that this is due to fertilizers.

The argument is that people in modern states are not fit. Death has been delayed by surgery and remedies of relief, but actual ill health has increased. Many more unfit people are, in short, being kept alive longer. This is one of those generalizations which it is difficult to prove, and its acceptance or rejection must be largely a matter of opinion. Personally I am for accepting it. I think it is increasingly true. The humus school, supported by a number of medical investigators, blame all this upon faulty diets, faulty foods. This is another generalization that must again rest upon opinion. This time I doubt the generalization in any total sense, for there seem to be many other factors in modern life which are likely to lead to sub-health. We live in a period of history when vast material changes have taken place all too quickly, when the complementary economic and social changes have lagged far behind. In 30 years war of the utmost severity has twice ravaged Europe; and between these two tragic spasms of an ill-adjusted civilization there have been several acute years of economic uncertainty with peak depressions condemning millions to an unnatural state of worklessness. In days when security was never more easily attainable in a material sense, the common man has lost even the illusory security of his forefathers. Can anyone doubt that this strain, the perpetual strain of *fear,* plays a major part in causing this modern sub-health? Nothing disturbs digestion more than worry, and the general rate of worrying in modern life is surely higher than it used to be in more settled periods. I know there is an argument that vitamin B deficiency, which is set up by the white bread desire of the times, induces nervous strain, and thus it is possible to say that modern nervous debility is directly due to malnutrition. This line of reasoning might hold in better days, but in the period 1914–44 there is surely no need to look for vitamin deficiencies to explain why human beings worry! Intelligent people or imaginative people

have had every reason to worry in almost the whole of those 30 years, and they have plenty of reason still to worry for the end of the second war will hardly alter the fact that material progress is out-pacing social and economic adjustment. That is one general reason for lesser health. I suggest that a minor one may be found in the peacetime car habit which has for many people substituted sitting-down for exercise. Middle-class people who work in offices all the week cramp themselves inside cars at week-ends when previous generations walked or did a little running on a playing-field. The enormous increase in smoking (this is a fair point for a heavy smoker to make—I am not being narrow-minded), though probably largely due to the general worry of modern life, must contribute its quota to sub-health. Many doctors say that smoking is unharmful, but this covers moderate smoking, and the modern habit is not moderate. In short, food malnutrition is by no means the *only* cause of sub-health even if it is one of the major causes.

This may seem an unnecessary and digressive point but the humus school argues that *all* modern sub-health is due to food. What I am trying to suggest is that, even if the food was always good, these other factors—especially *fear*—would still cause some of this unfitness, and therefore the basic indictment against food is not by any means 100 per cent true. When the humus enthusiasts go even farther, and say that the main fault with modern food is that it is raised with chemical help and not exclusively by humus, they are extending an argument that does not even start off with 100 per cent truth into a special explanation that is far more tendentious. It is quite illogical, at the end of such a chain of assumptions, to say: "Look at all the sub-health of people today, steadily increasing *since chemicals were used to grow food!*" Analyzed, it is a most unwarranted suggestion, this idea that chemical nutrients and human ill health are directly and completely connected. Yet without close analysis, the argument might seem plausible.

In *The Living Soil* Lady Eve Balfour quotes the *Medical Testament* drawn up in 1939 by a committee of Cheshire Gen-

eral Practitioners. These doctors stress the increase in sickness of modern life and contrast this with the modern postponement of actual death. They attribute sub-health to perpetual malnutrition. They draw attention to the work of Sir Robert McCarrison in India, who traced the various general healths of Indian communities to their customary diets; thus, healthy tribes regularly ate good diets and unhealthy tribes were those whose traditional diets were nutritionally poor. The fit community was one whose foods were largely fresh natural foods. McCarrison then reversed the usual biological research procedure and fed rats upon diets corresponding with those of the healthy Indian communities and upon diets corresponding with those of modern industrial England, e.g., white bread, margarine, cheap jam, tinned meat, vegetables boiled with soda, etc. The former rats *grew well, there was little disease amongst them, and they lived happily together,* while the rats on the modern diet *did not grow well, many became ill and they lived unhappily together,* so much so that by the sixtieth day of the experiment the stronger ones amongst them began to kill and eat the weaker. "The diseases from which they suffered were of three chief kinds, diseases of the lungs, diseases of the stomach and the intestines, and diseases of the nerves, diseases from which one in every three persons, among the insured classes, in England and Wales, suffer. . . ."

This experiment of McCarrison's has never received the publicity which its fundamental significance deserves. The humus school, in drawing wider attention to it, have performed a great service. Here is the modern processed *cheap* diet exposed for what little it is worth, and side by side the diet of fresh, wholesome food is given the full credit to which it is entitled. From studying the effects of man's own nutrition, McCarrison developed the bad symptoms of civilization in rats.

However—and this must be stressed—McCarrison did not discuss chemical fertilizers or humus. His experiment only compared fresh food diets with civilized, processed food diets. And in their *Medical Testament* the committee of Cheshire doctors pass immediately from their quotations from McCarrison to a purely

speculative suggestion that what is lacking in this modern diet is something perhaps not present where chemicals have intervened in the cycle of fertility. They jump from a point that *has* been proved very brilliantly to quite another kind of claim that *has not* been proved at all. Thus, without a careful analysis of the total argument, evidence that is sound for one assertion might be taken to be also sound evidence for the further and unconnected assertion.

The humus enthusiasts are fully entitled to write as much as they like about nutrition, about modern people's denatured foods. White bread and overprocessed canned and packeted meals —these foods are not the backbone of any healthy community, and there is more than enough evidence for such an opinion. However, without any other evidence the humus school cannot fairly assert that the de-naturization of this faulty food is connected with the use of chemicals.

People must eat more fresh food. Their bread should not be made with flour from which the vitamins have been removed for the sake of color. It would be unwise to say that there should be no canning because canning is a method of preserving seasonal foods; but, when there is canning, it should be conducted, as it can be, with maximum regard to the retention of vitamin values. All this food, handled freshly and carefully in these proper ways, can (and *must* on the evidence given in this book) be grown with the assistance of chemical nutrients; there is not any substantial evidence to show that its freshness or its wholesomeness or its known vitamin values will be any the less for that. The nutrition argument and the fertilizer controversy are two entirely separate matters.

Sir Albert Howard, like McCarrison, lays great stress upon the health of Indian tribal communities. McCarrison attributes this general health to a fresh and wholesome diet. Howard adds to the recipe the fact that these native peoples produce their food on ancient agricultural lines by returning all natural wastes to the soil. This added fact is *not* evidence against chemicals. It is a pro-humus piece of evidence certainly. Evidence that is in favor

of something somebody does cannot be regarded as evidence against some other action that he does *not* happen to do. These tribes, living in a selfsufficient kind of isolation, may well be able to produce their food needs on the amount of humus manures they can obtain; also, they have not to contend with the effects and demands of industrialization, and perhaps they are very lucky, or perhaps in the course of time it will be found that they have missed something. Living in an ancient traditional manner, not having developed the wants and crazes of modern man, they can arrange their agriculture in this way. It is certainly true that their superior health suggests that they do better without processed foods, but nothing suggests that they do better without the fertilizer drill.

At this stage I must admit to a certain amount of unfairness in presenting the humus school's nutritional argument. So far I have confined the issue to the terms of their own general thesis. To avoid a confusion of issue, I have not yet admitted that this school has put forward one definite and specific piece of evidence, and has made other efforts not quite as definite to present further pieces of practical evidence. These more factual attempts to prove the thesis must now be examined. They have been withheld until now because it first seemed necessary to show that the antifertilizer argument cannot be established by theoretical and assumptive generalizations.

One experiment has been recorded in which rats were fed upon (a) seeds of clover and grass from fertilizer treated land, and (b) seeds from naturally manured land. The natural manure details were pig manure with straw at 20 loads per acre, which presumably corresponds with about 15 tons. The chemical treatment was 20 hundredweights of basic slag (grade unstated), 3 hundredweights of kainit, and 1 hundredweight of sulfate of ammonia per acre. It is not possible to compare the NPK values of the two treatments precisely, but, taking an average value for the manure, on that side of the test the supply was about 1.8 hundredweights nitrogen, 1 hundredweight phosphoric acid, and 1.5 hundredweights potash. On the chemical side the slag (at an

average value) might have given 2 to 2½ hundredweights phosphoric acid, the kainit would have given 0.4 hundredweight of potash, and the sulfate of ammonia 0.2 hundredweight of nitrogen. It certainly seems clear from this, then, that we have this *standard* of comparison:

	Manure Supply	*Fertilizer Supply*
Nitrogen	1.8 parts	0.2 parts
Phosphoric acid	1.0 parts	2.0 or more parts
Potash	1.5 parts	0.4 parts

Even those who argue that NPK values do not matter very much when humus is used abundantly must surely agree that in a test of this kind some closer comparison than this is required. Even if, as we should, we halve the F.Y.M. figure for nitrogen because only 50 per cent will be active, it is still very much higher than on the chemical side. The potash figures, which represent the most important nutrient from the point of view of seed production, are badly disproportionate.

The test was a one-season test. The phosphorus content of the manure-plot seeds was found to be one-third higher than that from the chemical plot. This is surprising since it was phosphorus which alone of the nutrient trio was supplied more heavily to the chemical plot; though it might be speculated that superphosphate as a quicker acting phosphatic fertilizer would have *put more phosphorus into the seeds.* The protein content was: manure-plot, 12.7 per cent—chemical plot, 11.7 per cent—here surprisingly alike in view of the wide nitrogen disparity in the compared treatments.

The rats fed on the chemical-raised seeds did less well than those on the manure-raised seeds; and, where rats previously had been on a deficiency diet, the curative effect of the manure seeds was much quicker than that of the fertilizer seeds.

It would be wrong to suggest that the criticisms of the control-of-treatment side of this test remove entirely the indications of the nutritional result. On the other hand, this test would have been worth a great deal more had the soil treatments been rea-

sonably comparable. As it stands, we have a result—which has not unnaturally been much stressed by the humus school—that opposes the results of the joint Rothamsted-Dunn Laboratories findings. We have, in short, not a clash of theories but of experimental results.

This test, carried out by Rowlands and Wilkinson in 1929, should be repeated and with the soil treatments redesigned. A third plot-treatment should be introduced in which NPK chemicals *and* half the quantity of manure are jointly used. Also, Rowlands and Wilkinson apparently worked on single plots; work of this kind must in all safety be carried out in triplicate to avoid the misleading intervention of some unsuspected chance factor on one plot.

It should be stressed that on the orthodox side the nutritional values of chemical-raised foods have not been assessed by chemical analysis alone; the Dunn Laboratories most certainly would have used the biological check before stating that they could find no differences. It should not therefore be thought that it is only the humus school who present evidence on a biological food-value basis. Nevertheless, if this experiment was widely repeated, and if the same trend was shown, it would again be prohumus and not antichemical because it is the old story of comparing (a) plenty of humus manure, plenty for all needs, with (b) chemical fertilizers alone. It might be added that the previous cropping of the ground in this test was: cabbages—potatoes—autumn wheat—grass seeds with the wheat—hay. With these preceding crops drawn from the soil, it is at least possible that the humus status of the test soil was not high.

Other directly comparative evidence is rather less definite. Communities such as schools are quoted where the general health has risen after a change from ordinary foods to humus-raised diets. Lady Eve Balfour, discussing one such case, says: "It may be argued that the remarkable improvement in health brought about by this experiment was due to the vitamin treatment and not to the altered method of vegetable culture." Here *may* seems a very understating word. For these are not really *experiments* at

all—the numerous variables are much too little controlled. A change is made from a mixed diet of modern foods to one that is predominant in home-grown and fresh foods—and health improves. This is understandable without our having to attribute any special qualities to humus manuring. The war has shown, despite some very severe rationing of certain food-types, that a diet change that makes people eat more fresh vegetables produces better health. One must certainly expect the same thing to show up in the case of a boarding-school where there is perhaps no war-strain and where, instead of a mixed batch of people of all ages and pasts, the human samples are all young children with greater growth needs and thus with sharper natural responses to protective foods.

Howard, pressing for more work of this kind in feeding isolated communities upon humus-raised foods, declares that such communities would become *islands of health in an ocean of indisposition.* He goes on: "No controls would be necessary—these will be provided by the countryside around. Elaborate statistics will be superfluous as the improved health of these communities will speak for itself and will need no support from numbers, tables, curves, and higher mathematics." This must be criticized. How, without the most careful controls, can we possibly tell whether any health change is due to diet differences or to different methods of food production? A community feeding upon fresh wholesome home-grown foods is to be compared with a mass of people outside all eating various foods of many kinds and no doubt frequently of poor nutritional kinds with plenty of processed and denatured foods. And no controls will be necessary! Fresh food diets will always beat the mixed diet of average modern life no matter how the foods are grown, and this kind of *experiment* would show the tested community healthier than *average* people whether chemicals or humus or indeed perhaps nothing was added to the soil. There is one basis of comparison only which can be admitted into the court of reason; comparison of groups of people under the following controlled conditions:

1. Similar general lives, habits, etc.

2. Both sets consuming the same diet of fresh vegetables, non-denatured cereals, etc.

3. One set on food raised by chemicals only; one set on food raised by chemicals plus humus supplies; one set on food raised on natural humus manures only.

4. Results not to be regarded as definite unless the comparative tests are carried out over a number of consecutive years.

With people, such an experiment would be extremely difficult to carry out—to ensure adherence to the fixed diets over the long periods necessary machine-guns might be required, metaphorical if not actual. It would be of little use for the investigators themselves to act as the test samples of humanity—health is not unconnected with the mental side of life, and the responses of the bodies might be affected by the excitements or disappointments of the experiment. For this reason I was entirely unimpressed when one leading humus school writer attributed improvements in personal health to changing over to humus-raised food. Here could just as easily be a case of health improvement brought about by the harnessing of the mind and spirit to a satisfying crusade, to a stimulating purpose. Degrees of positive health are very much connected with the non-bodily facts of living. Most crusaders for specific diets, even for odd crankish affairs that have long since been shown to be nonsense, have themselves been good examples of putting their ideas into practice. It is the crusade as much as the diet. Health is psychological as well as physical and nutritional.

To go back to this test. If it would be difficult with people, the trouble would be much less with animals; and a test of this kind is, I understand, one of the proposed items in the research program at the Haughley Research Farms. In such an investigation, provided that the humus-plus-fertilizer plot is regarded throughout as of equal sample value to the other plots, the orthodox school and the humus school could collaborate without basic misgivings on either side. The fact that this is likely to be done is further proof that Lady Eve Balfour stands alone in the humus school in realizing (a) that proofs are still needed for most of

the school's opinions, and (b) that the methods of investigation should include the complementary use of chemicals and humus.

Some other evidence, quoted by her in her book, however, must be criticized. Dr. Scharff, chief health officer at Singapore until the fall of Malaya, reported in a communication in the *Compost News Letter* his successes with an allotment scheme for some five hundred coolies. All the allotments were compost-manured. Remarkable improvements in the men's health were obtained between 1940 and 1942. Following an enthusiastic account of this venture, Dr. Scharff says:

"It might be argued that the improvement in stamina and health amongst my employees was due to the good effect of unaccustomed exercise or to the increased amount of vegetables consumed. Neither of these explanations would suffice to explain the health benefit amongst the women, children, and dependants of my laborers who shared in this remarkable improvement."

Why not? The women, etc., may not have had the exercise of turning compost heaps or digging, but surely they had the extra quantities of fresh vegetables. Since there is every reason to suppose that fresh protective types of foods form a better diet than one that is not much composed of these foods, why shouldn't the health improvement be attributed to this cause?—especially in an experiment which (unhappily) was cut short after two years. One is forced to conclude that the experimenter was prejudiced in favor of compost manures, and unconsciously stretched the deductions from his experience because of that bias.

And to some extent this seems a general trouble with the compost enthusiast. His faith is intense. He sees all things as evidence for composting. There is indeed a great deal to be said for this faith. If it doesn't really move mountains, at least a little more of it (in regard to humus) might have saved the removal by erosion of thousands upon thousands of acres of soil. The humus preacher has a lesson to teach wherever the role of humus is ignored. However, it seems to grow upon the preachers—and they expand their text. All improvements are hastily attributed to a compost

heap if one is adjacent, and all deteriorations or sub-standards are attributed to chemicals if an empty fertilizer bag is hanging about. There is probably no clearer example of this forced reasoning than the common assertion of the humus school that the erosion troubles just referred to are due to artificial fertilizers. The western *dust bowls* have often been directly blamed upon chemical NPK. Yet, as Dr. Crowther has recently pointed out, the average consumption of fertilizers in Kansas, Colorado, and Oklahoma, three of the worst states for soil erosion, was about 1 hundredweight of sulfate of ammonia, 15 hundredweights of superphosphate, and less than ½ hundredweight of muriate of potash *per thousand acres.* Never was so much attributed to so little! As Dr. Crowther observes: "The dust bowl was caused by too frequent plowing and not by too much fertilizer. The remedy will be found in more cover crops and leys, and, to establish these, much more fertilizer will be needed." Thus, while erosion is due to neglecting the humus factor in soil maintenance, it is not due to fertilizer practice, but, by dragging into their arguments this antichemical crusade, the humus school people reduce the sound case they had (in a pro-humus sense) to nothing short of an absurdity that bears little relation to fact, whereas orthodox soil scientists in the United States, accepting the idea that it is lack of humus that causes soils to erode, successfully get on with the job of saving what land they can by growing humus-making vegetation with the help of fertilizers. However, this is rather a digression.

So far we have considered human nutrition; for even the rat experiments mentioned were carried out in connection with human diet investigation. The humus school has brought forward other evidence that is solely concerned with animal nutrition, and this is no small aspect of nutrition; much of our own food depends upon stock and poultry so that their diet is a basic factor in ours.

Sir Albert Howard records that at Pusa his oxen, reared on fresh green fodder, silage, and grain, all humus-raised, were able

to rub noses with other cattle suffering from foot-and-mouth disease and yet remain uninfected. He found this experience repeated at Quetta during 8 years and at Indore during 7 years. I am not proposing to comment very much upon this evidence, which seems strongly positive. I am not qualified to discuss in any detail questions about foot-and-mouth disease. A counterargument to this Howard evidence appears in the letter by Pollitt quoted in the next chapter. I would only suggest that the freedom from infection might still have been secured had some quota of chemical fertilizers been required to supplement lesser amounts of compost manure. Howard in his various Indian plantations was always able to secure sufficiencies of animal manure and organic wastes and plenty of labor; also, as Sir Frederick Keeble pointed out, there is strong evidence that Indian soils are generally low in organic matter content.

From experience under British conditions Lady Eve Balfour has recorded cases of cattle deliberately choosing to graze upon compost-treated pasture rather than upon fertilizer-treated pasture. Howard has stated (in a letter to *The Medical Press,* May 23, 1945) that, if any average permanent pasture is split into three plots, with the outside plots manured with Indore compost and the center plot treated with a fertilizer containing sulfate of ammonia, then cattle later on will *graze the herbage down to the roots* on the compost plots but will only *lightly pick over the produce of the poisoned soil.*

I must confess that this evidence seems powerful. While to some extent one must question the merits of nutritional assessment by animals' displays of preference, it cannot be dismissed as only a slender indication. It seems highly important that such cases should be rigorously investigated at research stations. In a large number of cases plots should be compost-treated and fertilizer-treated and it should be checked whether this Gallup poll of cattle invariably or almost invariably points to the compost produce being preferred. I cannot help reflecting that some of the humus school's other attempts to compare manures and fertilizers have been loaded in favor of the former before they start:

(*a*) by abundantly large applications of compost that could rarely be achieved in universal practice, and (*b*) by somewhat inexpert fertilizer selection. It is vital that all aspects of such a directly comparative test should be looked after by impartial recorders who see to it that both compost and fertilizers are employed efficiently yet without favor. A research station is the place for such inquiry—and both compost enthusiasts and orthodox soil scientists should collaborate in advice to those carrying out the work. If this animal preference was indeed confirmed in a predominant number of cases, then the next step obviously would be to try to determine in what way or ways the favored produce was superior.

It must not be forgotten that on the chemical side of this issue there is a considerable mass of recorded evidence, especially in United States experiment stations' reports, for the beneficial use of fertilizers in grass growing and in raising fodder crops. The Jealott's Hill experiments of 1930 showing the increase in protein content through nitrogenous fertilizer applications have already been mentioned, and perhaps I shall be accused of unduly stressing one test. However, there is not as much British research on this subject as there should be, probably because of the peacetime (that is, pre-1939) preference of the British stock or dairy farmer to feed his beasts mainly upon imported feeding stuffs and only casually upon home-raised crops. Indeed, it was with some regret that I learnt recently that even at Jealott's Hill this line of research—the relationship of protein values with nitrogenous fertilizer supplies—had not been continued since the period round about 1930. Figures such as those quoted in the third chapter for that 1930 experiment would have been most valuable as long-term averages over five, ten, or fifteen seasons. It is not, I think, entirely irrelevant to point out that the British dairy farmers, severely cut off since 1939 from their imported feeding stuffs, forced to feed their cows principally upon home-raised fodder and grass, and allowed *only* unrationed nitrogenous fertilizers for their grassland (until mid-1946 when phosphatic fertilizers were freed too), have, despite these difficulties, produced greater

annual quantities of milk than in pre-war years. A good deal of this remarkable achievement must be attributed to fodder-crops from arable land, to silage as winter-feed, and to the weeding-out of poor milking beasts, but some of it, at least some of it, must be due to the beneficial effects of extra nitrogen supplied to the grassland as sulfate of ammonia or as "NitroChalk."

Fortunately there is much more research of this kind to turn to in America. Indeed, J. G. Archibald, Professor of Animal Husbandry at the Massachusetts Agric. Exp. Station, in a recent paper in *The American Fertilizer* referred to the effect of fertilizers in increasing the protein content of roughages as: "rather well known." Even if it is well known in America, I still doubt whether it is so well known in other countries. The same authority stated that at the Massachusetts Station they had been able "to increase by about 40 per cent the protein content of grasses, and nearly to double the yield of protein per acre, by the application of 400 to 500 pounds of *complete* fertilizer per acre." The necessity to supply reasonably balanced fertilizers to grassland, and not merely nitrogenous fertilizers, is surely both obvious and fundamental. Howard's evidence of cattle deliberately preferring the well-composed plot to the plot treated with sulfate of ammonia might well have its real explanation in the *single nutrient* supply of the fertilizer plot. A good compost manure is, after all, fairly well balanced from an NPK viewpoint, and it may be that the cattle find a lack of phosphorus (or even calcium) in the predominantly nitrogen-fed grass. The soil may not have been able to supply enough of the other nutrients to accompany the additional nitrogen supply of the straight fertilizer. In Britain, as has just been pointed out, restrictions throughout the war period made it impossible for complete or balanced NPK applications to be given to grassland, the phosphates and the potash were reserved for arable crops; so we have had, at any rate in my own opinion, the rather mixed blessing of a greater attention to the nutrition of grassland at a time when only unbalanced fertilizer applications were possible. So, when Howard makes his compost-fertilizer comparison, he is hitting out at fertilizer rec-

ommendations at a time when these recommendations have been limited by the facts of war and not freely based upon the full application of scientific knowledge.

However, we are tending to turn back on our track by redeveloping this point, and it is rather more useful to go forward and hear more evidence from the Massachusetts Exp. Station. In the same paper, Prof. Archibald reports that "in some work which we did . . . in the late thirties, we were able to increase the carotene (the precursor of vitamin A) content of grass 73 per cent on the fresh grass basis, 86 per cent on a dry matter basis, by application of an 8 to·6 to 6 fertilizer at the rate of 400 pounds per acre. These increases were the average results from over fifty samples of seven different species of grass grown on fourteen different plots." This type of American evidence is of world-wide significance since, on the whole, it is only in the United States that an intense attention has been paid to the use of large dressings of complete fertilizers on grassland; in most of the world's farming, grassland has been run *on the cheap,* as little as possible being spent on the crop because some sort of mixture of green vegetation will always establish itself somehow. Great advances in the world's meat and dairy farming are possible if notice is widely taken of results such as these from Massachusetts.

The record of phosphatic fertilizers as grassland improvers is a well-established story of success. The loss of the major minerals from grazing land is very high. Calcium and phosphorus are heavily retained by the animals in the formation of their own bodies and of milk; yet many soils that are permanently pasture-land are inherently deficient in phosphorus, and sometimes in calcium. Here, for example, is another American statement (from the United States Yearbook of Agriculture, 1939): "A study of 775 pastures in West Virginia and analyses of the soils showed that the most important factors responsible for the poor type of vegetation found there were soil acidity and lack of available phosphorus. Eighty-five per cent of the area was found to be in need of lime and 94 per cent was deficient in available phos-

phorus." It is at least known that the balance of minerals contained in plants is considerably dependent upon the balance of minerals in the soil that supports them; the subject is complex, and cases have been recorded where the addition of one mineral as an extra supply to the soil will lead to an intake by the plant of other mineral elements. A large number of variable factors influence each case in which it is attempted to relate the soil's minerals with the minerals of the ultimate crop. In this branch of scientific agriculture the *expert* might well be distinguished by his cautiousness in making generalizations.

In a recent paper by F. E. Corrie on calcium in plant nutrition a neat example is given of the kind of thing that has been investigated. "Cruickshank recorded 0.52 per cent calcium (peak figure) in the dry matter of an untreated poor pasture, as compared with 0.64 per cent for an adjoining comparable pasture which had received lime and basic slag, sampled on the same day. Calcium in the untreated pasture rose from 0.37 per cent in May to its peak figure, 0.52 per cent, in August and fell to 0.46 per cent in mid-October. In the treated pasture, it rose from 0.56 per cent to its peak, 1 per cent, in July and fell to 0.37 per cent in mid-October."

Phosphorus deficiency in animal nutrition is, however, more serious and more frequent than calcium deficiency. To quote the 1939 United States Yearbook again: ". . . the soil in certain parts of the world is deficient in phosphorus, and roughage grown upon such soil is also deficient in it. Cattle in these regions suffer from phosphorus deficiency when they are fed exclusively on home-grown roughage or when they are fed on such roughage combined with concentrates low in phosphorus, such as corn meal. . . . The effects of phosphorus deficiency on dairy cattle have been much studied in the phosphorus-deficient areas. . . . They consist of loss of appetite, often combined with a craving for unusual foods. The fertility of the cows is much reduced, owning to their failure to become pregnant when bred, and their milk yield is also reduced. In severe cases the mineral matter of the bones is depleted and fractures occur frequently. In less

severe cases, the phosphorus content of the blood is reduced. . . ."

Still, it is not easy to tie up this business of feeding cattle with the soil upon which they graze or with the fertilizers that are applied to that soil. Especially is this true of our own cattle since, even in wartime, a substantial part of their nutrition comes from imported feeding stuffs or from feeding stuffs raised upon other farms. To risk a generalization that somebody more experienced would perhaps not risk: it is perhaps true that fertilizers have so far been employed on pastureland to induce the growth of suitable grasses and clovers where, without fertilizers, these crops were very poor, or to maintain an economic cropping level where this was badly declining through nutrient removal, but that fertilizers have not so far been much used with the deliberate aim to increase a content in the crop of this or that essential mineral—for, when a nutritional deficiency turns up in the animals, it is put right, generally, by incorporating the lacking mineral in a feeding stuff. Until a great deal more is known with some certainty about this latter function, the indirect effects of chemical fertilizers upon cattle is a subject much open to controversial argument.

I am aware that all this is a rather vague defense against the exceedingly definite claims and accusations of the humus school about animal nutrition. It is probably much vaguer than it need be, but—as I indicated a few paragraphs above—it is not a subject that I feel qualified to discuss or to analyze at all deeply. I must therefore beat a strategic withdrawal according to plan, remaining content to present, while I run, a few samples of the evidence on either side. However, in the next chapter, which is mainly composed of other people's views, some of the opinions quoted will be found to refute the humus school's case about cattle feeding with enough vigor to satisfy any bloodthirsty addict of controversy.

Since the publication of the first English edition of this book, a few research results have come forward dealing with the effects of plant nutrition upon vitamin values of final crops. This sort of investigation is, of course, fairly new; it has, indeed, hardly

been practicable until reliable methods were developed for the very accurate estimation of vitamin contents. At the present time, therefore, we have to bear in mind that published results are based *only* upon one-season tests—and, as we have seen, the one-season test is not wholly reliable as a test for plant-growth and soil effects. The following results must be regarded merely as indications. In due time, however, this kind of research work will have a most important bearing upon the argument of this chapter.

From the Long Ashton Research Station (England), Dr. Pollard, Keiser and Bryan published a paper in 1945 on factors influencing the vitamin C content of tomatoes. One of the factors of comparison, for both indoor and outdoor cultivation, was the addition of compost manure to the general NPK treatment. Both for indoor and outdoor cultivation, the addition of the compost *reduced* the vitamin C content of the fruit. The average content without compost treatment was 25.39 milligrams per 100 grams and with compost it was 22.86 milligrams per 100 grams. For sugar content, however, no significant differences resulted with or without compost.

There seems, on the other hand, to be a fair amount of evidence that, in general, the vitamin C content of vegetables and fruit decreases as the nitrogen fertilizer supply is increased. This conclusion has been reached by independent groups of American workers (Jones, Van Horn, and Finch, Arizona Agric. Exp. Station; and Wittwer, Schroeder, and Albrecht, Missouri Agric. Exp. Station). From a practical standpoint, however, it must not be forgotten that the true measure of nutritional value is the *total* production of vitamin quantity *per acre,* and, even though a specific fertilizer might reduce the content of vitamin per pound of fruit or vegetable, because of an increased crop yield the actual production of the vitamin might be greater.

From the Georgia Agric. Exp. Station, fairly negative results were obtained by fertilizing potatoes with numerous mixtures of nitrogen, phosphorus and potassium, the measured variations in vitamin values being described as minor. Vitamin C content was

lower, however, with a treatment of nitrogen and phosphorus than with a treatment of phosphorus and potassium, thus again suggesting that the production of this vitamin tends to fall if the soil's nitrogen supply is raised.

CHAPTER XVI

OTHER OPINIONS

" 'There's more evidence to come yet, please your Majesty,' said the White Rabbit. . . ." *Alice in Wonderland.*

HOWEVER objective one might try to remain, it is impossible to write a book about a controversial issue, or about one that is treated controversially, without the intrusion of personal attitude and personal opinion. I have, in any case, been quite frank about my natural bias. Decisions connected with book-creating, what to put in and what to leave out, choice of sequence, methods of argument and presentation—these are apt to be settled by personal bias, and they are there all the time between the lines of print however detached the words might seem to be. So this chapter is going to be written by other people, other people who in most cases have also considered the humus-chemicals issue.

A friendly critic who saw the manuscript of this book urged me to omit this chapter. It was contended that some of the opinions I had selected for quotation were just as sweeping as the humus school statements I had particularly criticized in other chapters, that I was ceasing to argue the issue out logically and trying to reach a decision by matching one extreme opinion against another. My aim in this chapter is simply to show what other people think about the humus case, other people who do not accept it. It is up to the reader to decide whether these opinions are fair or not, whether they are sweeping or whether they are moderate. Witnesses must be judged by the jury and, if so far I have been a kind of counsel for the defense, I would prefer in this chapter to be regarded as a *clerk* to the *court.*

It would, of course, be only too easy for the humus school to match each of these opinions with inverse views from the composting crusaders, but this has already been done in the numerous humus school books and articles. In the expression of opinion—and by this I mean vivid expression of the kind that reaches the layman rather than the restricted audience of science—the humus school has certainly not been backward. Perhaps only by chance, perhaps more subtly because the Nature-knows-best philosophy of this school tends to appeal to those whose leanings are somewhat artistic, the humus enthusiasts have on the whole been more entertaining *writers,* more attractive book-creators, than the orthodox scientists. To criticize the thesis of a book is not to deny its merits as a piece of writing, and who could deny praise on this account to *Agricultural Testament?* I certainly feel that the humus school has had its full quota of self-expression, and I think that this symposium of opinion on the other side, far from being one-sidely unfair, is overdue as a balancing factor in the controversy.

The first quotation comes from Viscount Astor and B. Seebohm Rowntree's, *British Agriculture,* Penguin Books, 1939, revised edition. Their earlier first edition was criticized for paying scant attention to humus school views. This extract from the second edition expresses the views of the committee of people carrying out the inquiry on which the book is based:

"Some have criticized us for not having dealt adequately . . . with what is called *biodynamic farming* or the *Indore process* or the importance of humus or organic manure. We have accordingly made a point of consulting some of the most representative and independent authorities on the subject. . . .

Now, no one would deny the desirability of maintaining a proper organic content of the soil, and that humus plays an important part in preserving this. But to admit this is far removed from accepting the statement, that human disease is increasing and that this is due to food having been grown in soil where artificial fertilizers have been used, or to food having been grown in soil where no natural manure has been applied.

The evidence to substantiate these claims seems inadequate and certainly all the vital statistics seem to indicate that disease among human beings, far from increasing, is decreasing. There is no doubt that crops grown in soil very deficient in certain minerals will become deficient in these constituents, and animals fed exclusively on these crops will in their turn become deficient and may become ill if these constituents are lacking. This is, for instance, true for aphosphorosis in horses, cattle, sheep and goats, a disease known all over the world in animals living on crops deficient in phosphorus.

Bad farming obviously will lead to a loss of fertility of the soil. But it has not yet been proved that the use of *compost* necessarily denotes good farming or better quality food.

Compost adds plant food, e.g., lime, phosphorus, etc., and adds humus which helps to retain moisture. But any artificial means of supplying these nutrients in the form of inorganic substances might have the same effect upon the plant as these nutrients supplied in the form of compost, and any kind of material which would help to retain moisture might have the same effect as the cellulose in the compost.

Experiments have been conducted at Rothamsted in which wheat was grown on three plots of ground which had organic manure, chemical manure, and no manure respectively. No difference in the quality of the grain could be noted, though this does not necessarily mean that the results would be the same if the experiments were carried out for a longer period.

While, of course, there is much still unknown, there seems to be no evidence of any mysterious substances or quality in humus which would affect the health value of plants other than nutrients which could be applied by the ordinary commercial fertilizers.

It is to be hoped that some scientific body will study and report on the whole question of the use of humus and that, if necessary, further experiments will be carried out. . . ."

Perhaps a *clerk* to the *court* is not allowed to say things to the jury, but at any rate I have seen them whisper advice to the

magistrate's bench, so let that be my excuse for comment. I think this committee's opinion decided rather too definitely against the humus school. The effect of humus upon the biological functions of the soil, the bacteria and fungi, seems to have been largely missed; and Nicol in 1937 had drawn attention to the possibility that such things as growth-promoting substances may well be associated with the decomposition of organic matter. Nor would it seem desirable to rely only upon the *vital statistics of disease* to assess health. There is more evidence than this committee apparently considered to suggest that sub-health, the cold and the gastric disturbance, is not decreasing. I suspect that if these experts reconsidered this issue now they might shift ground a little closer to the humus ideas than in 1939.

This 1939 committee inquiry is only one of many attempts to bring a panel of experienced opinion into the fundamental planning of agriculture. The war has at any rate made agriculture more fashionable. In 1944 the Parliamentary and Scientific Committee, which is described as an unofficial group of members of both Houses and representatives of certain scientific and technical institutions, went into conference on much the same task. A report of very great value, *A Scientific Policy for British Agriculture,* was issued. Paragraphs 36 and 37 from this Report summarize most admirably a balanced point of view in regard to the supposed clash of fertilizers and manures.

"36. Ever since the British discovery of artificial fertilizers about a hundred years ago, these substances have been increasingly used to supply some of the additional plant foods made necessary by the increasing withdrawals from the soil resulting from more intense production. Practical farmers are, however, undoubtedly right in their tenacious faith in organic manures; that is, farmyard manure, crop residues, and other kinds. Science is no less insistent on their importance. Artificial fertilizers are not a substitute for, but a supplement to farmyard and other organic manure. The proper use of fertilizers does not poison soils; it enriches them. Certain fertilizers, if misused, can do harm by introducing soil acidity or forcing up a lush and soft vegeta-

tion. But crops, like animals, require a properly balanced diet, and a better understanding of the correct use of fertilizers is undoubtedly necessary.

"37. The highly complex part played by organic matter in the soil is, however, still only imperfectly understood. It may truly be said to constitute one of the greatest of all the ultimate problems of the use of the land all over the world; possibly it is the greatest of these. To ensure ample research on soil organic matter in its fullest sense should be regarded as an essential of our post-war policy. This research should be of the most fundamental character, and actively directed to the great recognized problems of agricultural production and to the nutritional value of foods as influenced by soil and husbandry. The efficient utilization of urban waste products in connection with agriculture should also continue to receive close study."

The whole of this book is in agreement with this opinion; and these two precise paragraphs seem to summarize the point of view at which all these chapters have tried to aim. Thus, one chemically biased person making an individual and isolated survey of the evidence, seems to have come to the same conclusion as a group of people, some of them scientists and some of them politicians. I would add that the book was completely written before this report was made publicly available, and that this chapter has had to be amended in order to insert this reinforcement.

Also in 1944, attention was paid to the humus-chemicals controversy in the pages of *Country Life*. An article on fertilizers was followed by one of Sir Albert Howard's vigorous interventions, and this in turn was followed by some equally vigorous letters. The authors of two of these letters have kindly given me permission to quote them. First, from a letter from Lt.-Col. E. Parbury:

"*Artificial* as applied to fertilizers is what A. P. Herbert would call a witch word. That is to say, it is used to suggest something quite different from its real meaning. If we use the word in its

true sense, is not the whole farming procedure, from beginning to end, an artificial business?

The selection of crops that cannot possibly compete with nature in the raw, the reclamation and cultivation of the land, the continuous battle against the inroads, depredations, and freaks of nature, liming, rotation, harvesting instead of leaving crops to rot on the ground, the manufacture of humus by any process; all these are artificial procedures. . . .

Many farmers and gardeners discriminate between an organic artificial and a *raw chemical* (another witch word). What are these raw chemicals? They are all the products of Mother Earth or of God's good air. Some of them are of organic origin and some are nothing but the salts of the earth that nature has drained from the land and thoughtfully stored for further use. In many cases the only processing is designed to eliminate objectionable matter and to put them into a convenient form. Not one of them is a raw chemical in the true sense of the word. . . .

A reasonable interpretation of the Rothamsted experiments during the last 100 years may be somewhat on the following lines:

(a) Good farming takes the first place, and this includes the maintenance of humus by giving back to the land all the produce that does not go to market.

(b) Proper rotation is essential, among other reasons to balance the withdrawal of plant nutriment from the soil and to discourage the cumulative effect of soil-borne disease.

(c) The crops should be suited to the soil.

(d) Additional fertilizers produce satisfactory results when used with discrimination according to crop requirements and the nature of the land.

(e) The indiscriminate use of any fertilizer, including artificials, leads to trouble.

(f) Over-deep plowing gives evil results.

(g) The drawing conclusions from unconfirmed observations or short-duration trials is dangerous.

Now I propose to offer some suggestions on a few of the points raised by Sir Albert Howard in his interesting article. . . .

The trouble on large-scale mechanized farms may be connected with any of the many changes that have directly or indirectly affected the land in the last 100 years. Mechanized deep plowing may be a contributory factor.

Blight is not soil-borne. The disease is carried on from year to year and from place to place in infected tubers and becomes epidemic through wind-borne spores which attack potatoes whether artificials have been used or not. The worst attack occurred long before the general introduction of artificials.

The great failure of the coffee in Ceylon also occurred before the general introduction of artificials.

What evidence is there that *cattle diseases* result from artificials? Some of the best and the worst cattle in the world are to be seen in India; the best on the Government experimental farms, and the worst in the villages where farming practices have hardly altered in a thousand years. Many of the worst epidemics in this country were imported from abroad long before artificials came into general use and most of them have been stamped out or kept under control.

Nature the supreme farmer. . . . The main thing we see in nature's farming is that vegetation predominates according to its capacity to tolerate prevailing conditions. We find vast areas growing restricted varieties of useless vegetation and still vaster areas where nothing will grow at all. Anyone who has tried to grow garden plants on wild land knows that nature wins every time. On the other hand, one has only to leave one's land unattended for a short while and nature takes charge, whether artificials have been used or not. When one plants seeds under artificial conditions, say in the John Innes compost, anything up to 100 per cent will germinate and flourish. But a single foxglove growing nature's way scatters a million seeds and, likely as not, not one will germinate. The main lesson we learn from nature is that we must suit our crop to prevailing conditions or adjust the soil to suit the crop. . . .

What is the evidence that artificials kill *earthworms?* My own lawn has been dressed with artificials for years. Some of the weeds have gone, the grass has improved, but there is a fine crop of worm casts.

. . . Sir Albert Howard writes that artificials are always followed by failure. Then why have artificials become so popular? It is the obvious success and apparent latitude that leads to their indiscriminate use. Over-doing it with farmyard manure can also cause trouble.

Chinese methods. China has nearly the highest death-rate in the world. In Japan they conserve their sewage, as one knows only too well when passing through the villages. The death rate is 50 to 90 per cent higher than that of civilized countries where artificials are used."

I should explain that the above quotation is not quite the whole of Lt.-Col. Parbury's hard-hitting letter, but what I have omitted concerns particular points raised by the preceding article by Sir Albert and not the general lines of the argument. In a private letter Lt.-Col. Parbury describes his attitude as that of an experimental horticulturist rather than of an authority upon soil fertility or farming. Over many years he has made and maintained gardens in many climates and privately carried out tests with fertilizers and other chemicals; his one special subject of study being the uptake of aluminum by hydrangea plants. I hope he will not mind this slight publicity about his private horticultural life, but I think it is important because it shows that his views upon the subject of soil fertility are those of an experienced practical grower and therefore not the views of someone with a bias toward this theory or that.

Another letter in the same battle came from Lt.-Col. G. P. Pollitt, to whose book reference has already been made. Again only general points made in the letter are quoted:

"The desirability of the maximum quantity of humus in the soil is disputed by no one. To assume, as Sir A. Howard does, that a user of artificials would not apply all the humus he can economically obtain is of course not in accordance with the facts.

To suggest that potato disease is the result of using artificials ignores the fact that the worst outbreaks of potato diseases have occurred in countries, e.g., Ireland, where . . . artificials were wholly unknown. Thanks to the chemist thousands of lives have been saved in Ireland by the use of simple copper sprays.

Eelworm disease has as much to do with artificials as with the phases of the moon. It arises from the introduction of the pest in contaminated material and from growing the crop too often on the same ground and would occur whether or not this practice was followed, no matter how much compost (even of Indore manufacture) was used.

Sir Albert states that the use of sulfate on the land kills the earthworms. Is there a shred of experimental evidence to support this statement? There is ample and conclusive evidence to the contrary.

Sir Albert states that liability to foot-and-mouth disease comes from the use of foods grown with artificials. How does he reconcile this with the fact that the cattle areas of the Argentine have never had a ton of artificials in their history, but the disease there is endemic and almost the whole cattle population suffers from it? The only proved source of infection in this country is imported uncooked meat, and no bovine animal has ever been known to resist the disease if exposed to infection. The animals in India for which Sir Albert claims immunity were of course immune for other reasons, probably owing to their having had the disease in a mild form earlier. Foot-and-mouth disease is generally a comparatively mild disease in hot countries.

The chief reason there is more cattle disease today than in the past is no doubt that there are more cattle, more markets, more shows, and therefore more opportunities of spreading disease. . . .

Farming without humus in the form of dung or compost is the necessary result of economically unbalanced world agriculture. The dust-bowls of the United States and Canada are the result of the exhaustion of humus and all other fertilizer ingredients in the soil, not of artificial fertilizing. These areas have received practically no artificials. The trouble is largely due to

erratic and insufficient rainfall. Where there is sufficient rainfall to grow good crops annually chemical fertilizers are used effectively and *dust-bowl* conditions are unknown. To attempt to grow crops in *marginal* areas, as they are called, is unwise—to blame fertilizers for the results is nonsense.

Sir Albert attributes the gradual deterioration of the potato crop in the Holland division of South Lincolnshire to the use of artificials. If the facts are as he puts them the reduced yields and lowered resistance to disease are due to the exhaustion of the humus content of the soil. The use of artificials enables crops of 12 tons to the acre to be grown still. Without artificials the land farmed as he suggests this area is farmed would by now produce no crop at all.

Sir Albert holds up for our ideal the farming system of China. This would mean the abolition of water-borne sewage and the introduction into this country of the terrible diseases which afflict the unfortunate Chinese. Is there any other country where the soils are so foul as in China? Many millions of Chinese are suffering from intestinal worms and other pests found in the soil, some of which penetrate the feet of those working on it, e.g., bloodfluke, hookworm. Sir Albert can have little experience of China, while those who know Chinese agricultural conditions intimately realize how much China suffers from the insufficiency of organic matter and the misuse of sewage. The yields of most crops in China are low; e.g., the wheat yield is less than half that of Great Britain.

I do not think the large cities of Britain will give up water-borne sewage. Something might be done with the recovery of sewage sludge. If all that could be recovered were recovered, it has been stated that 1,000,000 tons per annum might be made available. On the 33,000,000 acres of agricultural land in Great Britain and Ireland this would provide a dressing of some 70 pounds per acre per annum. Of this type of fertilizer at least 150 times this amount would be required to replace a proper dressing of F.Y.M. It could very rarely be worth the cost of collection and distribution.

The Indore method . . . would entail at least doubling the number of workers on the land and enormous areas of compost pits would be necessary. It is not clear how this would be better than the present method of treading straw in cattle yards.

The contest between artificials and humus which Sir Albert says will be settled by time and experience exists only in his imagination. There is no such contest. Humus is of course essential in all forms of agriculture. Where owing to large concentrated populations inorganic material is removed from the soil as food and run finally into the sea as water-borne sewage it must eventually be replaced. Soluble plant nutrients leached out by rains must today in some countries be replaced by artificials if the land is to grow any food at all.

The majority of the vast masses of the people in India, China, and South-Eastern Europe are undernourished because they do not restore to the soil by means of fertilizers the plant nutrients which are removed by the meager crops grown.

The much higher nutritional standard in this country and Western Europe has been obtained by the use of both inorganic and organic fertilizers. If Sir Albert Howard's proposals were put into effect our standard would rapidly fall to the Indian and Chinese level and ultimately millions would inevitably die of starvation. . . ."

Lt.-Col. Pollitt, the author of the above letter, is not only a highly-reputed authority upon agriculture and soil fertility; he is a farmer who practices what he preaches. His book, *Britain Can Feed Herself,* recommends the use of chemical fertilizers at much higher rates than those in general practice today. I understand that Lt.-Col. Pollitt applies these very heavy dressings to his own land and obtains remarkably good results. This does not mean that he ignores organic manures, either in his book or in his own farming. The agricultural plan in his book, as discussed in chapter seven, included provision for a considerable production of F.Y.M. And, in a private letter, he expresses the opinion that "farming for high yields of grain is impossible without composted straw or other similar organic residues in whatever way

they are produced. I am myself composting this year some hundreds of tons of straw chaff carvings, etc."

Some useful evidence and expression of opinion appeared in the *Ministry of Agricultural Journal,* August 1944. A practical article by Mr. A. E. Brown, who farms 2,200 acres on the Isle of Wight, included the following comments upon artificial fertilizers:

". . . I do not hold the popular belief that fertilizers of organic origin are necessary as plant food. In my view such belief is biologically unsound; plants take up minerals, not organic substances, and the plant is Nature's agent for converting the minerals into the organic material which is necessary to sustain animal life. Moreover, whether fertilizers are applied as organic or inorganic substances, they have to be converted into mineral compounds before they become available as plant food. It is the mineral element on which the plant lives, and this being so, it is a little difficult to follow the arguments of those who seek to show that healthier crops are grown with dung than with fertilizers . . ."

This opinion, of course, is more pro-chemical than any views expressed in this book. I would not have quoted it had not Mr. Brown brought practical evidence with it to show that he was not arguing only upon theoretical considerations. Mr. Brown's farms represent the complete antithesis to the humus school farms. He continues:

"For the building up and maintenance of fertility, therefore, we here depend almost entirely upon purchased inorganic fertilizers.

And what about humus? Well, several hundreds of acres of the land now under crop have, by clearing and draining during the last 20 years, been brought under profitable cultivation. Under previous occupiers they had been allowed to revert to barren heath, gorse, bracken, bog and copse. Long experience has shown that provided good crops are consistently grown, the humus content of the soil can very well be left to take care of itself. For instance, last year a humus test was taken on two fields

of 50 acres which deliberately had been given nothing but inorganic fertilizers since 1928. The humus content was found to be 33.3 per cent above the average for that class of land. This land had been farmed by the previous owner on the four-course system with folding sheep, but neither the fertility nor the humus content was abnormally high."

It is not possible to regard results on one farm or one group of farms as sufficient proof that *the whole* of the humus school's argument is without foundation, and I have not quoted from this article for such a sweeping purpose. The point that can be made is this—if one practical farmer can over some 20 years of experience, use artificials almost wholly and *not* find that his humus stock rapidly dwindles or that he gets bad results, then at least it seems to show that the gloomier antichemical views of the humus school are unsound.

In the same number of *Agriculture,* Lady Eve Balfour's thesis in *The Living Soil* was examined in a review. The reviewer makes similar points to those made in the last chapter in regard to the humus school argument that ties up evidence about malnutrition with a theory about fertilizers. If the following quotation is only a repetition of criticism already made, it is included to show that my opinion is not isolated but one held by others.

"One chapter . . . quotes extensively from the *Medical Testament,* a publication issued by the General Practitioners of Cheshire, wherein it is affirmed that the root cause of much of the nation's ill health is faulty diet. This is, of course, an established fact, but it is a red herring drawn across the line of the book's main argument. The doctors apparently refer to a *hypothesis* that organically manured food crops may be more health-promoting than inorganically manured, but they adduce no evidence to support it. In the context of this book the first impression which the reader receives is that a responsible medical body is using the fact of widespread malnutrition to advocate the exclusive use of organic manures for food production. This *Testament* is referred to frequently as evidence of the superiority of organic manure; but it is not evidence—it is purely unsupported opinion."

In the issue of *Agriculture,* March 1940, Sir John Russell surveyed the compost manures. This very concise article should be studied by anybody seeking information on the making of compost manure; very often the practical information about composts is given by those who are overenthusiastic or by those whose experience lies in countries where the task is much easier; in such accounts the snags are apt to be understressed because the writers are overconcerned with the propagandist side of their ideas. However, concluding his account of practical methods of composting, Sir John Russell commented as follows upon the value of composts:

"In view of the great amount that has been talked and written about composts, there has been surprisingly little field experimental work on the subject. There is little doubt that well-made compost has considerable fertilizer value, but actual figures are almost entirely lacking, and therefore no comparisons with artificial fertilizers or farmyard manure are possible, nor are figures of cost under English farm conditions available.

A good deal of compost has been made on tea-estates in North India, where the necessary vegetable matter is easily collected from the uncultivated land near the estates. The collection of this material has, however, in places led to bad soil erosion.

It is stated that the results are best when sufficient quantities of cattle or other animal manure are available; they are said to be less satisfactory where the animal manure has been deficient. Attempts to run tea estates on compost alone, however, proved unsatisfactory; it was necessary to provide the proper artificials.

There has been much discussion among tea planters as to whether green material should be worked into the soil as green manure or made into a compost. On this subject, definite and good experiments have been made by H. R. Cooper, at the Assam Tea Research Station, and by T. Eden, of the Ceylon Research Institute. In no instance was there any evidence of the advantage of composting, and tea planters are now, therefore, recommended to dig in their green manures. . . ."

Thus, Sir John Russell's analysis of the evidence about com-

post manures differs very considerably from that of Sir Albert Howard, and there is a marked difference in statement of results in regard to tea estates. So far as the point raised about costs is concerned, this has been partly met by Lady Eve Balfour in her book published since Russell's article. However, the costings given by her assess labor at a 3 pounds per week wage, and they cover only the immediate work on the farm and not any transport costs such as would inevitably be incurred in the acquiring of material for farm-scale composting. However, since mechanical methods must intervene in any serious attempt to compost for farms, perhaps no significance should be attached to the costings of non-mechanized methods. In any case it seems better to decide first of all just how much composting is required from the point of view of fertility maintenance and sound farming; some lesser figure following orthodox views or some very large figure if the humus school thesis is accepted. Then, having settled this, costings would have to work themselves out by force of necessity; for in no industrial process can costings be divorced from output considerations.

In the October 1944 issue of the *Royal Horticultural Society's Journal*, Dr. E. J. Salisbury, C.B.E., F.R.S., late Professor of Botany in the University of London and Director of the Kew Botanic Gardens, contributed an article on organic manures and mineral fertilizers. His opinion must be regarded as particularly valuable for quite obviously his experience lies not so much in the field of quantity production as in the much more delicate field of specialized quality production. The botanical garden and botanical research must impose upon manures and fertilizers quite different conditions and requirements from those of farming and agricultural research. Dr. Salisbury decided against the humus school's thesis in no uncertain terms and the utmost place he would allow for organic matter was as a complement to fertilizers.

"Some people imagine there is some *special virtue* in farmyard manure or composts, or indeed in all that category of organic substances that are comprehensively referred to as *muck*—a *spe-*

cial virtue that, it is claimed, renders plants grown with these healthier and less susceptible to disease, and, moreover, it is even asserted that the value as human food of crop plants grown with so-called *natural* manures is high, whereas the use of so-called *artificial* manures is claimed to be prejudicial both to the health of plants and those that feed upon them, whether animals or humans.

"Whilst no one with any knowledge of soils would deny the value of adequate organic material, there is no evidence worthy of the name for the exaggerated claims just referred to. . . ."

Dr. Salisbury's paper is particularly concise and it is difficult to pick out any one point in his argument without requiring to quote the whole of it. Here is his conclusion:

"It will be realized, from what has been stated above, that the presentation of manurial problems as a controversy concerned with organic manures versus mineral fertilizers is due to confusion of thought and complete failure to apprehend either the facts or the problem. Seen in its proper relation to the mineral nutrients the organic fraction is in no sense a substitute for them but a means *inter alia* of rendering them more effectively available.

Each renders the other more effective, and though mineral nutrients are indispensable for the growth of plants since they are essential raw materials out of which vegetation is manufactured, yet without the organic material or other colloidal medium the efficiency of the nutrient supply is liable to be impaired and the maintenance of a balanced soil economy but wastefully achieved."

Here then from the horticultural heights of Kew is an opinion that refutes the humus school thesis rather less moderately than the evidence and deductions of this book. No one who has read Dr. Salisbury's book, *The Living Garden,* could think of him as not being a most acute student of soils and plants and the last kind of man to indulge in hasty generalizations.

Dr. Crowther, whose work as head of the Chemistry Department at Rothamsted has made him a greatly respected authority

upon soil science throughout the country, has in his paper on fertilizers for the Bath and West Society commented upon the connection between quality and method of plant nutrition as follows:

"Every farmer knows that crops may vary in value according to the conditions under which they are grown, and that a great deal of skill is required to ensure quality as well as quantity. Unfortunately, it is becoming fashionable to discuss *quality* as if it were some transcendental attribute to be attained only by following a prescribed ritual. The word *quality* should be scrutinized carefully; as applied to a crop, it cannot mean much more than *suited to its purpose.*

For crops used in industry, market conventions can be checked and developed by research, and the effects of manuring tested both by buyers' valuations and more objective data. For barley the nitrogen percentage on dry weight is an important factor, and field experiments have shown that neither the price given by skilled valuers nor the nitrogen percentage is appreciably altered by moderate dressings of nitrogen fertilizers, which greatly increase yields. . . .

For fodder crops, hay and pasture, the principal factors are the proportions of protein, readily digestible carbohydrates, fiber, vitamins and minerals, especially calcium and phosphorus. All these factors can be determined in tests on the effects of fertilizers, and there is abundant evidence to show that quality can often be increased by judicious manuring and management. There are large areas in the United Kingdom and vast regions of the tropics where fodder crops and human foodstuffs are of low nutritive value through lack of available plant foods in the soils. . . .

There is no universal definition of *quality* and no simple formula for obtaining it by manuring. The more romantic compost enthusiasts argue by analogy from the known importance of vitamins in animal nutrition and of hormones in plant and animal physiology, that similar materials ought to be important in feeding plants. Biochemical research progresses rapidly, and it is con-

ceivable that some day we must add certain complex organic molecules to our lists of essential plant nutrients. Cases may possibly be found in which fertilizers and manures affect the vitamin contents of crops. The contemplation of these possibilities and the search for them are, however, very different matters from assuming the results in order to advertise special practices. No critical experiment has yet shown any appreciable difference in vitamin content or nutritive value between crops grown with fertilizers and with farmyard manure."

So much for a collection of other opinions that have from time to time been expressed in this *controversy*. I do not suggest that any carefully selected list of partial statements can constitute a total or even a fair argument. All that I wish to show here is that most people who support the case for chemical fertilizers admit the complementary necessity for humus in some form, and that this is as far as they are prepared to accept the humus school case; that a number of practical people consider that the controversy is far more rightly called *artificial* than the fertilizers.

From quite a different point of view, one more witness must go into the box. I have not sought his permission because at the moment he has several other matters on hand. This time the witness is a statesman, Joseph Stalin; for he has twice in his speeches in prewar times referred to fertilizers—in 1934 at the 17th Congress of the Communist Party of the Soviet Union.

"One of the most effective means of increasing the yield of industrial crops is to supply them with fertilizers. What is being done in this sphere? Very little as yet. Fertilizers are available, but the organizations of the People's Commissariat of Agriculture fail to get them, and when they do get them they do not take the trouble to deliver them on time to the places where they are required, and see to it that they are utilized properly. . . ."

Later, in the same speech:

"It is to be explained, first of all, by the fact that the industrial crisis has affected every capitalist country without exception . . . and by the fact that the agrarian crisis has grown more acute in this period, and has affected all branches of agriculture . . . it

has brought about the reversion from machine labor to hand labor . . . a sharp reduction in, and in some cases the complete abandonment of, the use of artificial fertilizers. . . ."

I have not quoted from Stalin for any winged purpose, right or left. The point is frequently made that it is the profit-motive of chemical industry that has provided the power for fertilizer researches and subsequent fertilizer recommendations, etc. These quotations demolish this argument. For what vested interest or profit-motive existed in the U.S.S.R. in 1934? All industry belonged to, and was controlled by, the state. Any useless employment of labor or materials could only make a loss for the state. No private interest could *push* fertilizers. Yet here is Stalin scolding his agriculturists for not using the fertilizers produced by the state's chemical industry. Of course Stalin is no more an authority upon soil fertility than any other statesman, but his policy and his publicly expressed views must be regarded as those of his technical advisers. Yet, if this humus-only policy was wholly right, where else and where better could it be carried out than in Soviet Russia?

This point, indeed, is clearly brought out in the second of the Stalin statements, where he pointed to the decline in the world use of artificial fertilizers as a symptom of the world economic crisis. In Stalin's ideological view, the farmers' economic inability to put back into the soil what had been taken out was an evil of capitalism. Well, state communism may or may not be the solution, and this book has no ideological terms of reference anyway; but the humus school is fond of comparing continents, of instancing China against Britain or America, so that it is at least interesting to consider the Russian attitude toward chemical NPK. When *vested interests* are abolished, it would seem that the fertilizer idea still remains.

CHAPTER XVII

IN PERSPECTIVE

"A half-truth, like a half-brick, is always more forcible as an argument than a whole one. It carries further." The late STEPHEN LEACOCK.

THIS BOOK might have been written from two other angles, and both of these would have been less polemical. One would have been to treat the whole matter entirely from the point of view of known plant needs and to ignore altogether any schools of thought and their soil-handling ideas. The other would have been to set out quite objectively the humus school ideas and orthodox ideas, indulging neither in criticism nor in judgment. I had intended to adopt the second course at least to some extent, but such detachment was—for better or worse—not my kettle of fish. When it came to describing ideas for which I do not believe there is sufficient evidence, my objective instincts vanished. So this book has had a point of view, an angle; though all the time I have done my best to make this clear in the hope that people will study other accounts as well before coming to opinions of their own.

The humus school will undoubtedly feel that I have often gone out of my way to attack their thesis, that my Part One is too pro-chemical and my Part Two is too antihumus. However, since Sir Albert Howard and Lady Eve Balfour have presented so force-fully and lucidly their case for humus and against chemicals, I do not think it would have been very progressive merely to summarize what they have said for Part Two of this book; a detailed analysis of what they have said is more justified because, so far as I know, this has not been done elsewhere except in isolated articles and reviews. Still, this analysis has been critical, and

even critics have consciences. A footnote must be appended. It would be wrong for any one to think that the humus school has been barking uselessly up the wrong tree. On the contrary, the humus school has made a substantial contribution to agriculture and to the progress of soil science.

The thesis is not wrong—it is half right and half wrong; and the part that is right is just that part of agricultural practice which many were inclined to forget. Humus *is* essential for soil cultivation; humus manures are not exclusively essential. The humus enthusiasts have pulled us all up with a jerk in drawing such forcible attention to humus.

This may seem confused. All through this book it has been maintained that scientists have always recognized the necessity for humus. Why then did it need Sir Albert Howard and his followers to fly the humus flag higher? I think the answer is to be found in the psychological atmosphere of soil science and soil-tending practices. Scientists are not very good at *putting things over;* they are much better in their research stations than on platforms. They have inadequately stressed the role of humus in their advices and recommendations; they have said it is necessary, but too often as a kind of safeguarding afterthought, their punch and details having been reserved for NPK. I feel quite sure that no responsible scientist has ever intended to make a Cinderella out of organic matter or humus, but, by devoting so much more attention to NPK supplies, most of them have inadvertently let this happen. Similarly, and though as I have tried to show quite a lot of the fertilizer advertising has stressed the humus need, there has been far more publicity for fertilizers than for manures. After all, fertilizers are manufactured for sale; most manures are made on the farms for use and there is no primary commercial transaction in the operation.

In *Soil and Sense,* Michael Graham made these remarks:

"It is also noticeable that there has been a change in the written advice of successive generations of scientists. It used to be, *artificials instead of dung,* from some men at any rate, whereas nowadays we mostly read, *artificials with dung.* If this goes on it

may be *dung of course* by 1950, and *dung, not artificials* one day."

Surely Graham was really judging not so much what had been said or intended, but what had happened psychologically. There has not been a real change of front on the part of science. If an increased amount of reference to humus came along, it was because scientists were realizing that their understressing of humus, their greater attention to the newer fertilizer idea, had led to misinterpretation. They had taken the humus partnership too much for granted, and people had been led to believe that humus was only a sleeping partner to be dispensed with on most occasions. Perhaps in the early days of fertilizers there was indeed a tendency to drop humus altogether, but this must be forgiven as a normal growing-pain of progress—we get the same kind of wishful over-claiming today for most new discoveries and it takes a little time before limitations reveal themselves.

As a corrective to all this, the work of the humus school, led by Sir Albert Howard, has been of major importance. And it may have been no bad thing that the claim for humus was so sweeping. The attack upon fertilizers, the insistence upon humus alone, these have compelled attention where possibly a more moderate argument might have been largely ignored and wasted. Those who argue that the humus school have raised a controversy that does not actually exist may be logically right—obviously that is the point of view of this book. This is not a logical world, and the humus school have rescued a most important Cinderella by this artificial controversy which logic would disallow. Today moderation is at an appalling discount. The man who, out of sheer fair-mindedness and decency, hesitates in an opinion because there might be two sides to it, is regarded as nice but soft, charming but ineffectual. Products are distributed in peacetime and even to some extent in wartime far more by the skill of pressure advertising than by the consideration of actual merits and utility. Ideas and even the policies of government at times of crisis are accepted or rejected according to the merits of the publicity campaigns that go with them. If Sir Albert Howard

had not believed that humus should be used exclusively, many less people today would be convinced that it was even needed partially, and much less composting would have taken place in the corners of gardens. Nor would influential inquiries into the future of agriculture today be recommending large-scale state-sponsored research into the functions of humus and organic matter.

To the humus school this acknowledgment will be a kind of back-handed compliment. Nothing is more certain than the fact that these people all believe fiercely in the humus-only thesis and they have never presented it as a subtle method of compromise bargaining. Their faith is absolute. A general acceptance that humus is a copartner of chemicals would not be regarded as a success by Sir Albert Howard. But for recent modifications the humus school's venture in research at Haughley was so planned that compromise could not even be tested; and even now the comparison of *fertilizers-only* with *manures-only* is regarded as the real issue.

Nevertheless the practical benefits of the oversweeping humus claims have now been achieved. Few today do not acknowledge the humus necessity, and the compost heap is no longer despised. The time has come for the orthodox scientist to help in the humus school's work and for the humus people to share their expert knowledge of composting technology with the research stations. Should this book play any part in bringing these two schools of thought together, in quenching *controversy* and introducing cooperation, then its choice of a polemical method of presentation will have been justified.

CHAPTER XVIII

LOOKING AHEAD

"My own feeling is that we should guarantee to every man, woman and child the right to have enough milk and butter, enough fruit and vegetables, enough of the protective foods of all kinds so that everyone can be fit to do his part in the world of tomorrow." HENRY MORGANTHAU, Secretary of the U.S.A. Treasury.

THIS ACCOUNT of soil fertility principles and practices has dealt largely with the past, and this has been unavoidable since the aim has been to show how these principles have been established. However, in an age of such rapid technical change not even agriculture can stand still, and it seems worth a few final pages to take a speculative glance ahead. Will future tendencies in agriculture make it easier or harder to maintain fertility?

So far as the immediate future of our own agriculture is concerned, the prospects for better fertility are hopeful. Not for many decades have so many people been thinking about the future of agriculture, and today it is not only those with a stake in farming who wonder whether *cheap food* is really as cheap as its price ticket. This is perhaps merely a wartime phase of opinion. However, peacetime man may yet learn from history; for, although it is said that he never does, the history of the world since 1914 has been so intense and overpowering that it may this time drive its lessons home into the skulls of the unteachable. Children who have been burnt twice are likely to be more careful with fires than those who have been burnt only once. A more settled postwar agriculture should emerge after this second world war.

In any case, there are reasons of expediency that must affect

the issue. The entire world population will have to be better fed after this war. Guns and butter are intimately connected. The division of nations into *Haves* and *Have-Nots,* and the further division of single nations into nourished and undernourished classes, these are the background conditions of war, the stage and the scenery and the plot-theme of the whole ruinous business. The actors and the stage managers who eventually organize the actual performance are subsidiary—they follow the conditions. There can be no peace if so many millions under one flag have social security and vitamins while somewhere else more millions under another management (or even the same management) have to endure insecurity and scurvy. It is easy enough to say that peace is indivisible; proper nutrition and security are also indivisible. It is easy enough to consider that the 1939 war arose from ideological conflicts and leave explanation at that, but the ideologies themselves arose from maldistribution of the world's resources. Sooner or later—and it will have to be sooner if a third war is to be avoided with any certainty—the world must get down to the job of arranging international distribution of essential goods so that the *Haves* do not destroy and restrict while the *Have-Nots* struggle to produce substitutes.

I do not think it is tendentious to say that today most agricultural scientists are in favor of more diversified farming in the future, more rotation and less mono-culture, and greatly increased production of the vitamin containing foods. In the end it is the scientific opinion which will prevail because the gap between science and practice is being bridged from both sides, by increasing *popularization* of science on the one hand and by increasing educability of younger farmers on the other.

It is not only food that comes from the soil. Cotton, jute, flax, etc.—these meet other needs and demands of man than those of his stomach. It is not a very dangerous prophecy to say that in the future there will be an increasing pressure for *industrial* cropping; scientific developments are showing that, by special processing, many simple products of the soil can be used as important raw materials. Cellulose can now be extracted with some

efficiency from many plant materials; this can be used as cellulose or it can be further processed to make sugar, but this sugar might well be no addition to the larder for it can be fermented into industrial power alcohol. The world's future technology will be far more concerned with organic raw materials; milk as a raw material for umbrella handles was but a token.

Sir Albert Howard has not confined all his hitting-power to chemical fertilizers. In 1940 he drew attention to the necessity for balancing the demands upon Indian soil of the hungers of the machine and the stomach, for balancing the *money* crops and the food crops. The famines in India have since underlined his words. Whether we wisely balance it or not, this industrial demand upon the soil is likely to increase, and the world consequences might be serious from the humus angle. It should be easy enough to increase the production of NPK fertilizers; all would indeed be well were it only a matter of more nitrogen fixation and more working of mineral deposits. However, the machine crops tend to leave behind less organic residues for the soil than the food-crops. Certainly cellulose extraction processes seem to take and use all that can be provided, and it is cellulose that has hitherto been one of the major components of compostable wastes. This point was brought out very clearly in a German book of 1938, *Verwertung des Wertlosen* by Ungewitter, a book which revealed many of the *salvaging* processes by which the Reich sought selfsufficiency in preparation for the great gamble. The German chemists had tackled the surplus straw problem. German straw production was stated to be 40 million tons per annum. In 1938 only about 150,000 tons were being used for cellulose extraction, and about the same amount for straw pulp to make cardboard and paper. Great expansion in these processes was indicated in the book, so much so that even then it was being pointed out that farmers could economize in their use of straw and use peat instead for litter material. The German account ends with the prediction that a new era in cellulose consumption is approaching and that it can be assumed that straw will be a much more important source than wood as wood takes

80 years before it can be used for cellulose extraction. This is admittedly only an indication, but the ideas of German chemists need never be despised. Straw today is one of the leading organic wastages. Yet straw tomorrow may not be an available waste at all.

In the ordinary press and in the scientific journals there have been many accounts of what new technical developments might do with crops easily grown from the soil. The processing details and the economic aspects of these propositions are frequently stressed, but there is seldom any reference to the effects of this extra cropping *upon the soil.* Until all peoples of the world can be adequately fed without any loss in soil fertility, it would seem unwise to throw extra industrial demands upon the soil even if dividends from industrial crops appear to be higher than those from food crops. It is not so much the NPK fertilizer supply which would be strained—the query is humus.

APPENDIX

CROP NEEDS

THIS IS a rather ambitious collection of practical information; indeed, from some points of view, too ambitious. To correct this, let it be said at once what it cannot do. It cannot give precise amounts per acre of specific fertilizers for this or that crop. Fertilizers vary, soils vary, though the individual crop needs might perhaps be regarded as fairly constant. The quantity of fertilizer required per acre depends upon the inherent plant-food availability of the soil and upon the quantity of farmyard or other manure also applied. Nor would any formula fertilizer application apply to all kinds of growing, for aims of growers vary too. The market-gardener may be aiming at an early crop rather than a big crop, the farmer may be concerned only with the yield. There are, in short, far too many individual variables in all these matters for tables of applications to be set out.

It should be remembered that, as general principles:

1. Soil rich in decomposed organic matter is likely to be fairly well off for nitrogen, but soil rich in undecomposed organic matter is likely to be poorly off, temporarily, for nitrogen.

2. Heavy soils hold a much better potash reserve than light soils.

3. Animal manures are usually relatively low in phosphoric acid, and many soils tend to be deficient in this plant food.

4. Animal manures are slow in action, especially in regard to nitrogen, so that it is necessary to underestimate the immediate nitrogen value of any F.Y.M. added.

THE CEREAL CROPS

The principal nutrient for cereal growth is undeniably nitrogen but unless nitrogenous applications are balanced with phos-

phorus in particular and potassium to some extent, either from the soil's reserve or from other fertilizer or manure additions, troubles due to nitrogen excess are likely to arise, e.g., lodging, too much straw, readiness to become diseased. The fact remains that surveys showed, before the war, that about 60 per cent of our cereal crops were grown without nitrogen applications at all; and where the moderate dressing of 1 hundredweight of sulfate of ammonia per acre was given, a million pounds spent on the fertilizer yielded additional produce worth 4 million pounds. So, as a general recommendation for the cereals, a one-hundred-weight per acre spring dressing of one of the chemical nitrogen suppliers should be regarded as standard practice. If more nitrogen is needed than this amount, then (to express a personal opinion with which some authorities will disagree) it should come from better nitrogen-providing husbandry, e.g., manures, legumes in the rotation, etc.

With this general recommendation covering all the cereals, here are details about the usual cereal crops individually.

Barley

Soil needs: Not suited to very heavy soils, nor to very light soils. Soil acidity likely to be fatal. If there is any doubt about this, liming must accompany pre-treatment of soil for sowing.

NPK needs: Liberal phosphatic applications advisable. Moderate potash helpful. Nitrogen at the rate of 1 hundredweight of sulfate of ammonia (or equivalent) should be given to increase yield except where the soil is known to be richly nitrogenous.

In view of the antiacid factor so marked with barley, basic slag is a most suitable phosphatic fertilizer to apply, provided it is applied early.

Wheat

Soil needs: Medium to heavy soils in fairly dry districts suit wheat best. Acidity unfavorable; also light soils or soils ***in poor heart,*** i.e., in very low humus status. With very richly organic

soils a variety should be chosen which will most resist lodging.

NPK needs: For soils other than those in rich state of fertility, liberal phosphatic supplies should predominate. If sown as winter wheat *early,* moderate nitrogen application is useful to help the crop *get away* before any risks of bad frosts. Moderate nitrogen application in the spring will increase the yield.

Potash need is low, wheat-suited soil generally being able to supply sufficiency.

Oats

Soil needs: Crops well on most soils. Tolerant to acidity; can do well even at pH of 5.5. Soil with moisture-holding capacity in summer is ideal; therefore good humus status is desirable for best results.

NPK needs: Liberal phosphatic applications must predominate. Moderate nitrogen in late spring desirable. Moderate potash, especially on lighter soils, helpful to grain yield.

Rye

Soil needs: Poor light land, usually used because better soils suit other cereal crops. The least exacting of all the cereals. Can stand considerable acidity. Especially in the United States rye is grown as a catch-crop or winter cover crop to increase humus-status of soil.

NPK needs: Yields from poor soil can be greatly improved by fertilizer applications, though rye crops are often regarded as good self-foragers.

Fairly good phosphatic and moderate potash applications are repaid. There being less risk of lodging than with other cereals, generous nitrogenous application also pays, especially in spring.

ROOTS AND TUBERS

Sugar Beet

Soil needs: Soils must be able to provide a well-drained and deep seed-bed. Medium to light soil with good water-holding

power in sub-soil is most suitable. Lime status must be good. Humus status should also be good.

NPK needs: Heavy feeding crop. Manures and fertilizers both desirable. Generous applications of all three plant-foods are recommended. Nitrate forms of nitrogen have often given better results than ammonia forms. A compound fertilizer with good potash content is usual practice. Wartime tests have shown that 3 to 5 hundredweights of agricultural salt, applied before sowing, will greatly increase the yield of sugar. The sodium of the salt apparently functions as a direct plant food.

Potatoes

Soil needs: Grown on wide range of soils, but best suited to deep medium or light soils. Light soils in areas not liable to late spring frosts are best used for early crops. Acidity is useful rather than adverse; lime should never be given just before this crop. Humus status should be high for good results.

NPK needs: Manures plus fertilizers should be the rule. Twelve to twenty tons of F.Y.M. per acre commonly applied.

The NPK balance in the fertilizer application is a matter for variation according to individual cases. Generous phosphatic and potash applications are essential, and the potash need is higher for light soils. The problem is, how much nitrogen? Excessive nitrogen in proportion to the phosphorus and potassium may result in overgrowth of haulms and later in the boiling-black trouble. Where the soil is likely to derive a good nitrogen supply from the manure supply or from previously applied organic matter, e.g., plowed-in turf, low to moderate nitrogen seems advisable. At any rate, high nitrogen application should be decided upon with caution.

Potato fertilizers of compound kind, designed by experience for local conditions, are the safest fertilizers to use for this crop. This would seem to be recognized in farming practice, for many farmers who scorn compounds for other crops always order compounds for their potatoes.

Carrots

Soil needs: Deep non-heavy soils best suited. Lime status should be fairly good. Moisture retention important.

NPK needs: High phosphatic application, moderate nitrogen and moderate to high potash. Further top-dressing with nitrate-type nitrogen during growth often given. Several research reports, however, have shown that high nitrogen feeding leads to higher percentage of *splits,* and also to poor keeping quality.

Parsnips

Soil needs: As for carrots above.

NPK needs: Much as for carrots. Potash need perhaps less.

Turnips and Swedes

Soil needs: Deep light loams with good drainage. Lime status *must* be high. Heavy soils unsuitable.

NPK needs: If possible manures and fertilizers should be jointly supplied.

Very generous phosphatic supply, moderate nitrogen unless F.Y.M. has been supplied in which case this is not so important; and potash supply to a moderate extent especially for very light soils.

Mangolds

Soil needs: Not very exacting except that it cannot be grown on very thin soils. Lime status should be high.

NPK needs: Liberal manure if possible plus base and top dressings of nitrogenous fertilizers. Nitrate nitrogen often considered to be more effective. Base dressing of phosphatic fertilizer also required in most cases. Potash helpful. Liberal dressing of agricultural salt often beneficial to this crop. Salt and F.Y.M. before sowing, and a compound fertilizer with good nitrogen and phosphorus and moderate potassium at time of sowing, would be an excellent policy for high mangold yields.

LEGUME CROPS

Beans

Soil needs: Heavy to medium land most suited. Lime status must be satisfactory—acid soil fatal.

NPK needs: Some manure if possible. Base dressings of phosphorus and potassium with the latter on generous scale, but no nitrogen unless the crop seems to be slow in which case a small top-dressing might help. Legumes should always be left to develop their own nitrogen by the nodule bacteria fixation process. If, for some reason, this does not proceed satisfactorily, a failure might be remedied by quickly supplying some chemical nitrogen.

Peas

Soil needs: Lighter land than for beans. As field crops grown for stock feeding, beans would be chosen for heavy to medium land and peas for lighter soils. Lime status as for beans.

NPK needs: F.Y.M. considered to be non-beneficial for direct use with peas. Preferably the humus manure should have been applied to previous crop.

Fertilizers as for beans.

Vetches

Soil needs: Does well on most soils. Deep medium soil perhaps best suited. Lime status must be high.

NPK needs: Generous phosphatic base-dressing most influences yield and maturity. Moderate potash helpful.

Alfalfa

Soil needs: Suited to many soils. High lime status essential. Good drainage almost equally essential.

NPK needs: As for vetches.

Lupins

Soil needs: Usually grown on light soils for green manuring

and nitrogen enrichment. Probably suitable to most soils. Lime status not so important as with other legumes, but should be improved by liming if very low.

NPK needs: Moderate phosphatic and potash dressings will give better crops.

GRASSES AND CLOVERS

NPK needs: Ley farming has so wide a meaning that it seems best to leave out the question of soil needs. So far as fertilizer applications are concerned, the phosphatic needs probably predominate initially, and this feeding should be generous. The ley often being seeded with the final arable cereal crop, the question of which crop gets the lion's share of the fertilizer is bound to crop up. Basic slag is undoubtedly most farmers' choice for this crop.

Top-dressings of active nitrogen fertilizers at 1 hundredweight per acre should follow as spring cultivation practice.

On light soils results will be improved with moderate potash supply.

It is, of course, vital for the encouragement of clover that the lime status should be put right at the start.

OTHER CROPS

Onions

Soil needs: Rich soils of most kinds suitable, so long as a good seed-bed can be provided. Drought-liable soils not satisfactory.

NPK needs: Generous phosphorus, fair potassium, but not too much nitrogen. Beware of nitrogen excess, especially after bulb formation.

Flax

Soil needs: Medium drought-free soil.

NPK needs: Nitrogen should be kept low with phosphates and potash generous. Wartime experiences have, however, indicated

that the potash requirement is not as high as was previously thought.

Kale

Soil needs: Suited to a wide range of soils. Lime status must be high.

NPK needs: A hungry crop. F.Y.M. plus complete fertilizer treatment gives best yields. Generous nitrogen and phosphate with fair potash supply. Nitrate-type top-dressing at 1 hundredweight per acre at hoeing time also beneficial. Kale can profitably utilize plenty of nitrogen.

Cabbages

Soil needs: As for kale but dry soils less suitable.

NPK needs: As for kale and with even more attention to successive top-dressings of nitrogen during the growing period.

Rape

Soil needs: Widely suited, but does best on medium soils. Lime status should be high.

NPK needs: F.Y.M. plus moderate nitrogen with moderate to generous phosphates.

MARKET-GARDEN NEEDS

All the above recommendations or guidances are made with the farm crops in mind rather than the more intensively grown market-garden crops.

It would be difficult to lay down rules for this kind of cultivation. Much of the fertilizer and manurial practice is aimed at successive crops rather than at one crop per season.

However, the *balance* of crop needs will be the same whether grown on farms or in gardens. The market gardener will need to give bigger applications, and as was indicated in chapter nine this heavier feeding is often better accomplished by using the organics, especially in the case of nitrogen.

The following brief indications may clarify the cases of crops more or less confined to market-garden practice. Soil needs are not discussed for these become individual problems from garden to garden.

Lettuce

Nitrogen should be well balanced with phosphorus and potassium otherwise leaf-growth without hearting will occur. Lime status should be good. The nitrogen need does not come early so that there is much to be said for top-dressing rather than base-dressing with quick-acting forms, or for base-dressing with slower organic kinds.

Tomatoes

For the outdoor crop, moderate nitrogen and good phosphorus with high potassium. Indoors, nitrogen can be stepped up, phosphorus brought down—but high potassium still essential and in the *sulfate* form.

A crop ideally suited to the use of specifically designed compound fertilizers. A very satisfactory proprietary outdoor fertilizer has the analysis: nitrogen—4.0 per cent, soluble phosphoric acid—4.5 per cent, insoluble phosphoric acid—3.5 per cent, and potash—10.0 per cent. Excellent results have been given with this nutrient balance.

Excessive nitrogen *must* be avoided for outdoor cropping or leaf-growth will predominate.

Marrows and Cucumbers

Generous nitrogen and phosphates. Only moderate potash or hard fruit results.

Celery

For good heads, avoid excessive nitrogen. Generous phosphorus and potassium for quality.

Sprouts

Heavy feeding crop. High nitrogen and very high phosphates, with enough potash to keep the growth healthy. (This seems a summary of most recommendations, but there are various *local* practices, e.g., heavy use of nitrate of potash in one area for the early market, heavy use of soot in another.)

Lime status should be high.

Cauliflower

Generous phosphorus and fairly generous potassium seem necessary to control nitrogen benefits. Too much nitrogen in the balanced feeding may inhibit curd formation. A case for complete fertilizer treatment. Much seems to depend upon the suitability of the soil for this crop. High lime status required.

Fruit

This is too wide a subject to be discussed in the brief space of an appendix, but the following general rules might be stated.

Apply phosphorus and potassium generously in the winter, as it takes time for these nutrients, which move slowly in the soil, to reach the deeper root systems of trees and bushes.

Nitrogen should be applied in the early spring, but discretion should be exercised, e.g., it would be wise to omit nitrogen feeding for a tree already over-wooded and under-fruiting.

Fertilizer application should be complementary to other nutritional practice, e.g., F.Y.M. or compost, or grass cultivation under the trees. If the grass of an orchard is regularly cut, it should not be removed as this means that much of the soil's NPK is being frequently taken away. If removed, the cuttings should be composted and later returned as autumn or spring dressings around the trees. Otherwise the cuttings should be allowed to *rot* around the trees. The neglect of this factor is a major cause of NPK starvation in our orchards. The minor *cash* gain from hay cropping is no gain at all when offset against the loss of nutrients from the trees.

Additional Points

In the recommendations above, especially in regard to farm arable crops, generous phosphatic application would be equivalent to 3 hundredweights of superphosphate per acre or 8 to 10 hundredweights of basic slag of lower grade. Generous potassic application would be equivalent to 1 or 1½ hundredweights per acre of 60 per cent muriate of potash. For nitrogen, moderate application would be rather under 1 hundredweight of sulfate of ammonia or nitrochalk, etc., rising to 2 hundredweights per acre for generous treatment. But high rates of chemical nitrogen application should be adopted cautiously, and it will often be best to split the dressing so that half is applied as base and the remainder as top-dressing during growth. These actual figures of application are given here simply to make the qualitative terms: moderate, generous, etc., more tangible; they should not be regarded as rigid regulations, but should be varied with relation to amount of F.Y.M. applied, previous feeding of soil, and so on.

With farm crops, the most suitable crops for which manures should be directly applied in the rotation have been indicated above by special mention of manures or F.Y.M. Because some crops have not been mentioned, it does not mean that they do not require some supply of humus. The supply of organic matter or humus manures is not so much a crop practice as a general fertility practice. It is assumed in all these recommendations that attention is being paid to humus status.

As has been stated, rates of application in intensive cultivation will be greater than in farming. However, in doubling the farm rate for nitrogen, the intensive grower should remember that he must split his active nitrogen or alternatively bring in the less soluble organics. Overapplication of soluble nitrogen in single doses is probably a dangerous misuse of fertilizers.

BIBLIOGRAPHY

HEAVEN FORBID that anybody should base his knowledge of any subject upon a single book. Soil fertility is one of the widest of subjects, and very much deeper-rooted than such matters as social security or full employment which are today discussed more frequently. Here is a list of books from which to choose for further reading. This is in no sense a full list, but has rather to be looked upon as a personal selection. Comments are given as indications of the *kind* of book and they are intended to have a descriptive meaning, rather than to be at all critical.

TECHNICAL BOOKS

Sir John Russell, *Soil Conditions and Plant Growth*, Longmans, Green and Co., 1937.

Often known as the Rothamsted Bible. The most thorough survey of its kind. Rather heavy going for people not used to scientific literature of the *treatise* kind. Not devoted to NPK science in any exclusive sense, on the contrary, considerably devoted to the biochemical aspects of soil.

Russell and Voelcker, *Fifty Years of Field Experiments at Woburn*, Longmans, Green and Co., 1936.

Also heavy going, and the ordinary reader may find the masses of test figures confusing. A book worth borrowing from a library to get an idea of the immense amount of data collected in field tests.

Sir Daniel Hall, *Fertilizers and Manures*, John Murray, 1909.

Not quite such heavy going. A solid presentation of the orthodox fertilizer case. Indispensable in the library of the scientific student of this subject.

Sir Frederick Keeble, *Fertilizers and Food Production*, Oxford University Press, 1932.

A very readable book. Emphasis on nitrogen and on Jealott's Hill work. Covers a lot of ground easily and concisely.

United States Yearbook of Agriculture (United States Government), *Soils and Men*, 1938.

A magnificent symposium of research reports made possible only by the enlightened United States attitude to research. If the British Em-

pire, or just Great Britain, ever produces half as good a book of this kind—well, leave it at that. Although restricted to United States problems, most of the book has general application in principle if not in all details. A book written by over sixty specialists in subjects connected with soils.

T. Wallace, *The Diagnosis of Mineral Deficiencies in Plants*, H.M. Stationary Office.

Color plates to help diagnosis by advisers and some fifty pages of text. The text is admirable and covers much more ground than might be expected in the space. A new book which the scientific student should regard as an indispensable, and which the more general reader should consider tackling. Not at all heavy going.

H. Nicol, *Plant Growth Substances*, L. Hill, 1938.

Readable account of this subject, and not as complex as it looks, because Nicol refuses to ignore the ordinary reader in any of his writing. Rather chemical in spots, but the non-chemical minded can skip a lot of this without detriment to the rest of the book.

Firman Bear, *Theory and Practice in the Use of Fertilizers*, Chapman and Hall, 1929.

An American survey of fertilizers. Needs discerning reading because the story is told historically and distinction must be made between *dead* ideas and still living ones. Contains a good many test results. A good book for the reader who has first read several others. Not heavy going.

Gericke, *Complete Guide to Soilless Gardening*, Putnam and Co., Ltd., 1940.

A delightfully lucid account of hydroponics from all angles; based upon United States experiences.

E. C. Large, *The Advance of the Fungi*, Jonathan Cape, Ltd., 1940.

A book to treasure. Attention has already been drawn to its merits in chapter fourteen. A long book, but not a heavy one. Devoid of jargon.

E. M. Crowther, *Fertilizers During the War and After*, Bath and West Society.

Though of pamphlet type this fifty or so page booklet is full of information about fertilizers and manures. Quotations from it in the course of this book will have given some idea of its value as a record of up-to-date facts and experience.

In addition, both Russell and Hall wrote simpler books for agricultural students. Russell, *Soils and Manures*, Cambridge University Press, 1919. Hall, *The Feeding of Crops and Stock*, John Murray, 1937. The latter in three small volumes. These are textbooks which the practical *layman* could take in his stride.

GENERAL BOOKS

These are books aimed deliberately at the general reader with a keen interest in the soil.

Michael Graham, *Soil and Sense,* Faber and Faber, 1941.

A shortish book full of provoking inquiry. Tending toward the humus school, but more moderate than the humus school itself. Graham performs the same service to orthodox science that the intelligent *heckler* performs to politics—that is, he asks most of the awkward questions.

H. Nicol, *Microbes by the Million,* Penguin, 1939.

It may be out of print now. If so, try to get a second-hand copy. The best book for the general reader on the biochemical aspects of soil fertility, yet not by any means confined to the soil. A modern classic of popular science.

Sir Frederick Keeble, *Science Lends a Hand in the Garden,* Putnam and Co., Ltd., 1939.

Collected edition of gently written articles on gardening. Full of good things about the connection between science and greenfingers.

E. J. Salisbury, *The Living Garden,* G. Bell and Sons, Ltd., 1935.

A masterpiece of *science for the gardener* from the Director of Kew Gardens. Sub-titled *The How and Why of Plant Life*—and this fully describes the book. Delightfully readable.

HUMUS SCHOOL BOOKS

I hardly like to describe the humus school books as general and not technical, but it remains a fact that they have all aimed at the ordinary reader rather than at the scientific student or scientist.

The two main books have already been discussed.

Sir Albert Howard, *An Agricultural Testament,* Oxford University Press, 1940.

E. B. Balfour, *The Living Soil,* Faber and Faber, 1943.

Both of these books are stimulating, the former for its vigor, and the latter for its persistent persuasion.

F. H. Billington, *Compost,* Faber and Faber, 1942.

A useful practical book describing in detail various composting methods.

F. C. King, *Gardening with Compost,* Faber and Faber, 1944.

A very antifertilizer book, but good on composting ways and means. So far as the antichemical case is concerned, Sir Albert Howard and Lady Eve Balfour have done this much better.

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